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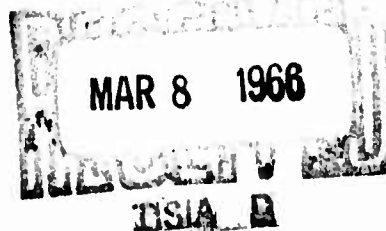
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THE FUNDAMENTAL RESEARCH ACTIVITY IN A TECHNOLOGY-DEPENDENT ORGANIZATION

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THE FUNDAMENTAL RESEARCH ACTIVITY IN A
TECHNOLOGY-DEPENDENT ORGANIZATION

A selection of papers presented at the Tenth
Institute on Research Administration held
26-29 April, 1965, Washington, D.C. under the
sponsorship of The American University

Howard M. Vollmer, Editor
Lawrence W. Bass
J. E. Goldman
Floyd Mann
Donald G. Marquis
William J. Price
Howard Reiss

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FOREWORD

The Center for Technology and Administration is pleased to cooperate with the Air Force Office of Scientific Research in publishing selections of papers presented at our Tenth Institute on Research Administration, held 26-29 April 1965. As in the nine similarly named Institutes held in former years, our object has been to stimulate thought on new avenues of improving managerial acumen in the research process.

The particular selections included in this publication were originally grouped in one of our sessions entitled, "The Fundamental Research Activity in a Technology-Dependent Organization"--organized with the cooperation of Dr. William J. Price, Executive Director, AFOSR.

The attendees at our Institute found this material of tremendous interest, and it is our belief that others who may now learn of its content will also be stimulated to apply these principles to their own experiences.

Ralph I. Cole
Director
Institutes and Special Programs
Center for Technology and Administration
The American University

INTRODUCTION

The purpose of this selection of papers, originally presented within the American University Tenth Institute on Research Administration, has been to discuss the role and the organization of fundamental research activities within the context of larger technology-dependent organizations in private industry and in the Federal government. Dr. Goldman's paper sets forth the basic role and rationale for fundamental research in such contexts, describing why it is needed and what it can be expected to accomplish. In doing this, he draws particularly upon the experiences of the Ford Motor Company in its Scientific Laboratory. Dr. Marquis follows to describe the characteristics of the scientists who are typically employed in these kinds of fundamental research activities--what their backgrounds, expectations, incentives, and career patterns are likely to be, drawing upon the many studies that have been made on this topic. He shows, particularly, how scientists engaged in fundamental research are likely to differ from those engaged in applied research or in engineering development.

Then Dr. Reiss provides more information on how fundamental research activities--and particularly what he calls "phenomena-oriented research groups"--must be organized and managed in order to accomplish their goals successfully within corporations such as North American Aviation, Inc. He draws attention to the different kinds of research done in such corporations, and shows how these different kinds of research activity require different principles of organization and policies of management. Dr. Mann goes on to report on findings from a study that identified significant differences between the characteristics of more effective and less effective units in a scientific organization within a Federal government agency.

Dr. Price describes how one research organization, the Air Force Office of Scientific Research, goes about utilizing the efforts and output of the extramural science-oriented community, in universities and elsewhere, to the benefit of the overall mission of the United States Air Force. He points out the importance to the Air Force of having this kind of activity, rather than assuming that it will be done by other government agencies.

Dr. Bass gave the luncheon address the day that these other papers were presented at the Institute. In his address Dr. Bass drew especially upon his experiences in studying the role of research in newly developing countries abroad. He pointed out the analogy between the role of fundamental research in these developing countries and in industrial corporations, reflecting an earlier mention of this analogy in Dr. Goldman's talk. Dr. Bass' remarks point to a danger in too much emphasis upon fundamental research where the resources of a country or an organization are too limited to support it, or where applied research and development are not yet sufficiently advanced to take advantage of it. There must be a proper balance between the different areas of science and technology.

The question and answer period following the formal presentations gave opportunity for a further discussion of several of these topics. The discussion focused on new trends in the education of scientists and engineers, the evaluation of research productivity, the role of the behavioral sciences and economics research, whether there is a current "disenchantment" with fundamental research in industry, and how to determine when, and the extent to which, fundamental research in industry is really necessary.

The panel of papers was conceived and put together by Dr. Price, with the close cooperation of Mr. Ralph I. Cole, the Director of this Tenth Institute on Research Administration. Dr. Vollmer served as the moderator for the panel, has assisted in editing the papers for this publication, and has provided some information on the consequences of the

organizational separation of research from development in large corporations, which is shown as an appendix herein. For the benefit of readers who wish to investigate in more detail the topics discussed by this panel, a selected bibliography has also been included.

As is true for most symposia in which papers are first given as lectures or talks and then later transcribed and published in written versions, the written versions of the papers appearing herein are not presented in a language that is as rigorous and grammatically pure as would have been the case if they had been prepared originally for written distribution. Nevertheless, it was felt that a written transcription of these remarks, essentially in the form that they were first presented to this Institute, would be of interest and perhaps of timely use to a wider audience--hence this publication of these remarks on "The Fundamental Research Activity in a Technology-Dependent Organization."

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I SOME BACKGROUND COMMENTS

Dr. Vollmer:

As really the least experienced member of this panel here today, they asked me to introduce the other speakers rather than to make a speech myself. However, yesterday afternoon I happened to slip in to the back of your room and to hear the tail end of some of your discussion. This stimulated my thinking a little bit, and last night in the hotel room I jotted down a few ideas that I thought might provide a little general background for our topic today. I have stated these ideas as four fundamental assumptions or propositions. These are general things which I think many of us know and are aware of, and yet perhaps it is well to state them as a backdrop, or as a stage setting, for the presentations which are going to follow.

Proposition number one is this--that the technical eminence of the United States today, I think, is traceable most directly to our historic background of native American capabilities in engineering development, rather than to our accomplishments in fundamental scientific research in this country. Historically, I believe that this has been true. We have been a nation of builders and inventors.

The westward movement, which shaped the character of American society as a whole in the 18th and 19th centuries and well into the 20th century, and the conquest of the frontier, called for quick, practical, and ingenious solutions to staggering engineering problems. So we developed in this country a long list of names of inventors, builders, and technical entrepreneurs which is unsurpassed in the history of any other modern country. Therefore, I think it is not too much to say that modern American society and our total culture--our whole way of life--is most

markedly what we might call an engineering culture, dominated by an engineering mentality and an engineering point of view.

As a people, I think we understand engineers, engineering, and engineering types of organization much better than we understand science. And this leads me to the second proposition.

Our native American culture in fundamental science, I think, is not as advanced as our native American culture of engineering. Historically, many of our ideas in science and many of our scientists were imports from Europe. In the 19th century, and even into the 20th century, European contributions to the physical sciences were more numerous and exceeded American contributions. Many scientists who finally settled and made their contributions in this country were immigrants or children of immigrants. And we know that this accelerated just before, during, and after World War II. Therefore, at least until Sputnik, scientists were still looked upon by many Americans as "foreigners" at best, or as "mad geniuses" at worst. And this leads me to proposition number three.

In America, I think that we need more advances in the fundamental sciences if we are to maintain our position of leadership, or even maintain a place in the front ranks of the nations in this present age of rapidly advancing technology.

In technologically less advanced eras, societies, engineering can advance, things can be built, new devices can be invented, with relatively little fundamental scientific knowledge underlying them. But not so today--we cannot afford to think that because we got along pretty well without using much fundamental science in the past, we can do so now. "Cut and try" methods are becoming more and more inadequate to the needs of the times. Pressures of population expansion, challenges for the exploration and utilization of outer space and the ocean depths, requirements for a wide variety of new synthetic materials, the need for less expensive and more versatile sources of energy, and the age-old riddles of how men can learn to live better with their fellow men--all these problems, and many

more, cry out for fundamental research in physics, chemistry, biology, and all the other basic sciences, and yes, including the behavioral and the social sciences.

We can't rely simply on the engineering genius of America to survive today. We know that the success of the Soviet Union's space programs since Sputnik attests to its scientific advances as well as to its engineering accomplishments. We see a resurgence of scientific efforts in old Europe, which may, even in this century, be coupled with the development of a United States of Europe that rivals the United States of America in every technical field. We also notice scientific advances in other parts of the world. Japan and Israel come to mind in this regard, for example.

Proposition four is that the successful pursuit of fundamental scientific endeavors requires a special kind of organizational environment. This is really what we are going to talk about here today, so let me state it again: Successful pursuit of fundamental scientific endeavors requires a special kind of organizational environment. We will have more discussion as to what this is.

Science is different from engineering, has different objectives, different value systems, different types of people involved in it, and it requires, therefore, a different form of organization.

American universities have in many cases, though not always, developed organizational arrangements, research centers, and institutes on campus, which are highly favorable to the pursuit of fundamental scientific research. Outside universities, some independent, nonprofit research centers like the national laboratories under AEC sponsorship have done this also, as have some government agencies, with varying degrees of success. Some individuals even dare to believe that there is a place, an important place, for fundamental research organizations within the larger context of technology-oriented enterprises in industry or government. And this, again, is what we are going to be discussing here today.

Now I would ask that you please note your questions down as you think of them for each speaker. We will have six speakers. We will have a general panel discussion period after lunch when all these gentlemen will be available to answer questions and to discuss points with you. But we would rather go through the talks before we have the questions.

The six speakers on this panel are all men who are experienced in three different fields or areas, if you want to call them that. First, as scientists themselves--they have all been research scientists. Also they have all had experience as research managers and they have all had experience in teaching at one point or another in their careers. Three are physical scientists who are currently directors of important fundamental research activities in industry or government. Two are social scientists who are currently directors of groups of studies on research management and research organization. The other has been a research director and is now a management consultant. All have written many publications in their scientific fields. All have important things to say on our topic. Although they may not always agree with each other, and you may not always agree with them, we hope that what we have to say here today will be thought-provoking.

II SCIENCE IN A TECHNOLOGY-ORIENTED ORGANIZATION

J. E. Goldman

Dr. Vollmer (Introduction):

So I would like to introduce our first speaker. He received his bachelor's degree at Yeshiva College and his master's and doctor's degrees in physics at the University of Pennsylvania. He has served as a research physicist in the laboratories of the Westinghouse Electric Corp., and has also been on the faculty of the Carnegie Institute of Technology. Since 1955, he has been Manager of the Physics Department of the Scientific Laboratory of the Ford Motor Company, and since 1962 has been Director of the Scientific Laboratory of the Ford Motor Company at Dearborn, Michigan. He has been a consultant and a member of many advisory panels connected with government activities. So with this brief introduction I would like to present Dr. Jack Goldman, who will speak on "Science in a Technology-Oriented Organization."

Dr. Goldman:

Thank you Howard. I would like to start with two apologies. First of all, the pressure of certain rather urgent business (and if time permits, I may even tell you what that urgent business was, because I think it's relevant to the subject of our discussion) has prevented me from being here the previous days of this meeting. I understand it was very interesting, and it may turn out that I will repeat things that have been said or take issue with things that have already been under discussion. So I apologize for that in advance.

The second apology I must make is with regard to my remarks. The following remarks and whatever I may say in answer to questions will draw on personal experiences. These personal experiences, naturally, would be

the ones I'm most familiar with, and this is why I draw upon them. Therefore, please forgive me if so many of these experiences come from that area with which I am most familiar, namely Ford's experience. The intention is not in any way to make a "commercial," but rather to talk about that which I know best. Right now I'll just dispense with one "commercial," however, by letting you know that the chairman neglected on introducing me to let you know that my affiliation is with the Ford Motor Company, "makers of the Ford family of fine cars."

Now, this morning we're discussing the problem of basic research in mission-oriented organizations. Dr. Reiss and I will discuss industry. In this regard we can ask four questions: (1) Why does a mission-oriented organization--especially an industrial one--do basic research? (2) What does it do? (3) How does it do it? (4) And who does it?

I will direct my attention to trying to answer the first two questions--"why" and "what." Dr. Reiss will be discussing the organizational problems. Therefore, in a sense he is answering the "how" and to some extent the "who." I too will touch on the "who."

Let me enumerate, first of all, several reasons--and I will not in any sense suggest that I'm enumerating them in the order of importance--but several reasons why an industrial enterprise should do basic research.

The first is what I like to call a defensive reason--to stay in the competition of the market place. We live in an age where there is so much science going on that--if you'll pardon the cliché--you never know where they'll achieve that scientific breakthrough that will render your product obsolete, or at the very least put you at a competitive disadvantage. Therefore, you must do a certain amount of research so that you smell what's happening and protect your investment, protect your responsibility to stockholders, and in the final analysis your responsibility to the public.

Now, there is a second reason which I call offensive--because if you are lucky enough, if you play your cards right, and if you do this in

proper fashion, as some organizations have learned to do, you not only protect yourself against technological obsolescence, but you might in fact do some of the inventing yourself, or maximize the probability that your own organization becomes the one that does the inventing, and thereby gain competitive advantage in the market place.

If there is an area which many industrial corporations--I dare say most industrial corporations--today look to research for, that is as a source of diversification. The most likely mechanism for diversification short of acquisition or merger is the hope that within your own ranks you will discover or invent new things which create either a new product, or a new market, or both. And so, research is a mechanism for this, and it is by no means a trivial mechanism.

There is a fourth reason that's often glossed over, and yet many of us consider this indeed as one of the most important reasons for doing research in a large industrial enterprise. This is to provide a source of high caliber personnel--technical personnel and ultimately management personnel. Today, industry depends more and more on technology. Almost any industry today has to be technologically oriented, and so there is a need in top management circles for technical people and technological input. One of the proven avenues of recruiting high level management talent into corporate structure is perhaps due to an accident of the last generation--many of the smart kids that have come out in the last 20 years have elected to go into science. And so if you want to get the smartest kids, you really have to provide them with an opportunity to come into the company via the research route. You can cite many examples in many corporations of top echelons of management who have been drawn from the research game.

In my company, for example, the present general manager and vice president in charge of the Ford Division, which is one of the most important executive positions in the company--this is the man whose name is quoted every day if you read the reports on what the sales in the last

ten day period is; I don't know whether you read them, but I always do!-- is Donald Frey. Donald Frey is a Ph.D. from the University of Michigan, in metallurgy. He was originally recruited into the company to do basic research in metallurgy, and this was his starting point for a rise, a meteoric rise in the company, because he happens to be brilliant. Now, of course, Frey not only enjoys such a responsible position, but you have to put a higher price on how much a company could afford to pay to recruit this kind of talent into the company. You are almost forced to the conclusion that it's worth the cost of the scientific research that you have done over the past ten years just to get this kind of a guy. It is certainly true in a company our size. Now that's only one example in higher management.

But there are other reasons why you want to recruit good people and to recruit them from the technical areas. There has been a lot of discussion, I understand, and I'm sure there will be more, on the distinctions and the contrast between what we like to call "basic" or "fundamental" research and "applied" research. It's another characteristic of this generation of scientists that the good ones all come out of school wanting to do nothing but pure basic research. They dream of the purity, and they don't want to taint themselves by touching things that are more applied in nature.

Well, I think our experience--and I'm sure Dr. Reiss', who has had so many years of experience at the granddaddy of all first class research laboratories, Bell Labs--will undoubtedly document the fact that the best way to get the best men into applied research is first to recruit them into the company to do basic research. Ultimately they recognize that they become sensitized to corporate needs or to national goals, and it doesn't take long for them to recognize that they can serve a very noble and useful purpose by doing applied work.

I'm fond of citing an example that we had. It has impressed the editor of Fortune magazine sufficiently that he's thinking of doing a

little article on this one case. We have a very, very fine physical chemist, one of the best in the country, who's working today in the field of combustion research. He is particularly concerned with the problems of hydrocarbon emissions in combustion, or what people like to think of as smog production. As you well know, this is a serious problem not only for our industry but for the country as a whole. Now, if you look around the country, you find very, very few first class people doing anything in combustion. In fact, one of the characteristics of the industry I'm in, and I noted this particularly when I entered it, is that, considering our investment and what a large fraction of the gross national product this product (motor vehicles) is, there has been remarkably little, in fact almost no, real good fundamental research on the chemistry of the combustion process.

And you couldn't go out for love or money into the academic halls and recruit a fine Ph.D. to come to work and do combustion research. The best ones have ample opportunity in the romantic industries or the glamour industries, and in the glamour fields of science. But here is a man whom we recruited from an AEC laboratory where he had done some of the pioneering work on the heavy metal fluorides which were so very important in the separation of uranium and plutonium, and he started out working in this same field when he joined Ford. But several years later what emerged is a recognition on the part of this individual that combustion is a very important problem. It is important to Ford, it is important to the country, and by golly it involves some interesting and good chemistry. And he realized that he could probably make a great name for himself, if he turned his attention to this. Now he is working full time in the field of combustion research. You would never have gotten him into the corporation if you had tried to interest him in this at first. I predict that out of his work, as sure as night follows the day, some important development may arise which will be important to us in this problem of the control of hydrocarbon emissions.

There is still a third reason why you want to use basic research as an avenue for recruitment. And that is, you always have the obligation to stimulate others. Again, this is the problem of how you keep within your corporate enterprise the best analytical minds and the sharpest brains sometimes doing things that are tedious, laborious, prosaic, and I'd use the word "pedestrian" if we in our company weren't opposed to pedestrians in all classifications! In that type of activity you may want someone highly qualified technically looking over others' shoulders. A well known, outstanding theoretical physicist can enjoy the admiration and the respect of not only his own colleagues but the members of the engineering fraternity who are across the hall and the development people who are across the street--they sort of welcome him when he comes in to chat with them about what's new, how we are doing, how we are solving this little hydrodynamic problem, problems about new compiling techniques for computers, and so forth.

There is a corollary to all these reasons for doing research. I can summarize it in the following form: At the present time there is a fifteen billion dollar annual volume of research being done in this country. That's a lot of research, and as you know, many speeches have been made, and articles written, on how to tap this resource. Research is going into such things as space and rocketry and high energy physics, and the problem is, with all the new science that's coming out of this, and the new technologies, how is the country at large going to tap this? With all this science and technology being pursued everywhere, how are we as a company going to evaluate this? How are we going to know what is going on and how are we going to put the right assessment, the right "fudge factor," on the kind of work being done in order to recognize something of value when it comes along? If you don't have your own people who understand this thing, you'll never be able to tap it. This, I think, is the essence of the point. If you don't have your own people, you won't tap the technology and you can't translate the technology for management.

You can't rise to the occasion when it bears on your product, and needless to say, you don't do any real sound innovating.

These are reasons why one wants to, and should, do research. So the next question is, what do you do to get there? I tried to summarize this in four terms which I will discuss briefly--I'm sure Dr. Reiss will elaborate on some of these and I suspect Dr. Price too, because it's all related to his situation also. There are really four essentials.

First and foremost is people. I think this is one of the clearly distinguishing features between "basic research" and "applied research." Basic research is people-oriented, not program-oriented or subject-oriented. I'm sure Dr. Reiss, in discussing the problem of tailoring an organization to do this, will concern himself with how you develop an organization which will focus on people. Let people germinate the ideas, let the ideas come from them. I'll leave this to you Howard, I'm sure this is something you'll probably talk about.

The second thing is atmosphere. Atmosphere must be conducive to doing research, to new ideas. How do I put it--by atmosphere I mean the many freedoms that competent research personnel require in order to render their actions conducive to creativity, the freedom to allow one's mind to wander across uncharted fields, freedom to go off on tangents, freedom for people to interact with each other, and this includes a system of rewards and motivations, even though research accomplishments may not be so tangible or comprehensive as those of the salesman or manager. The atmosphere includes such things as the proper facilities and the recognition of what kinds of facilities are required. After all, the research man needs facilities that are quite a bit different from what we are accustomed to ask our purchasing departments to go and buy for pilot plant, or for development, or for production.

The third is the problem of leadership. Basic research organizations in the final analysis depend upon a cadre of proven leaders--people of intellectual capacity who are the thinkers, the originators, the analyzers,

the synthesizers, the people around whom others will congregate. This is the way science has always been and undoubtedly will always be. Certainly, every Ph.D. that comes out of school thinks of himself as the guy who is going to win a Nobel prize, discover the transistor, and do this and do that. But, we all know that only a very small minority, a very small fraction, of these people will really be the creators and the originators. So part of the problem is to recognize this kind of leadership and to build your organization in such a way, and to motivate people in such a way that others will look up to these leaders. In this sense, I think, too many organizations make the great mistake of swearing by this bogey called "freedom." Everybody has to get freedom and that's it. Well, there are subtle ways of letting the Ph.D. who comes out from school and can think of nothing else than to clean up the corners of his thesis that he really ought to be looking to another person for guidance and leadership.

Now, of course, our problem is to get our fair share of these kind of leaders. I'm competing with Howard Reiss on this. We don't compete in the market place, really, but we certainly compete for people. I compete with Bill Price and I compete with the Bell Telephone Laboratories, General Electric, and IBM. One has to generate mechanisms and techniques for doing this.

And finally, the thing that in the long run spells the difference between success and failure in realizing these goals of research--of basic research when an industrial organization does do basic research--is the mechanism that I like to call "coupling." Bill Price has used this word extensively. Coupling is a very important thing. I like to think that the word "coupling" originated with the National Academy of Sciences committee five years ago which I had the privilege of chairing and studying the DOD research programs and research efforts in an effort to try to focus on their problems and possible solutions.

We raised the question of coupling in this committee. You can have a lot of good science and you can have a lot of good technology, but if these are not in contact with each other, if these have no overlap, no relationship, no recognition of each others problems, then science will never do the corporation any good, and development and technology will never tap the potential of a science. So this coupling mechanism is a very important one, but it is so important that it has to be done with kid gloves. It has to be done by people who are expert in this and who are trained and who learn the technique. In this regard I take my hat off again, as I have in so many occasions, both orally and in print, to the prototype of the modern, successful research laboratory, the Bell Laboratories, which has mastered this technique of coupling.

Coupling, like research, is something that is a function of the people who do it. You have to look for, and then recognize, the talents that a good coupler has, and make sure that he is moving along in the organization in such a way that he will be useful to the organization and that he won't bemoan for the rest of his life the fact that he is no longer at the bench in the laboratory soldering wires together and putting tubes or transistors in their proper place. When a guy tells you "Oh how I yearn for the laboratory" when he is sitting as manager of a development section, or director of a scientific laboratory, then he's misplaced. He should be happy and enjoy and be good at what he is doing so that he realizes his own fulfillment in doing that.

These are the four important things that help you do what you set out to do when you create a basic research enterprise in industry.

Now, how do we judge what kind of research we should support? Obviously, we can't run the whole gamut of research. It's only natural that an organization, or the management in an organization, will pre-empt some of the subject matter. You will pre-empt it by looking to those things which you can somehow relate to your ultimate goals or to your products and neglect, at least a priori, those things which have no apparent

relevance to your product. Thus, while we may term it "fundamental" or "basic" research, it's basic and fundamental insofar as it's probing the frontiers of pure science and the scientist is doing what he wants to do, but you originally selected the scientist because he was interested and competent in a field that you felt is pregnant for your own product or for your own goals. Nevertheless, if it's a good and vibrant laboratory, people will go off on tangents.

For example, we are working in cancer research at the Ford Scientific Laboratory. One or two scientists in our laboratory who had capability in a particular area of research--in this case it was magnetic resonance--had some ideas. They got all excited about looking at certain biological systems, and "X" months later, and after some input of time and effort and collaboration with some physicians at the Henry Ford Hospital in our area, out came a beautiful piece of research. Now the chemist involved is spending most of his time in biochemistry. Well, biochemistry at the Ford Scientific Laboratory is not something we would set out to do. But in view of the fact that the person who has done it is doing a fine job, is doing first class scientific research, we aim to keep him doing it.

Also we had hired a young man, a Ph.D., to work in the field in superconductivity. This is an exciting field; we think there are going to be a lot of things coming out, a lot of technology, a lot of good science, and our laboratory has made some contributions to this field. After six months this fellow got interested in general relativity. Now I tell you, in fact I promise you, that it's not in the cards in the next two decades for us to put out an antigravity car. We are not working on general relativity because we think there is going to be an antigravity car. We are working in this field only because of a wonderfully smart genius whom we were lucky enough to recruit and who got interested in it. People of the caliber of Dirac and Lancosz evaluated his work and considered it very, very highly. Therefore, if it is a good contribution to science we will support it.

There are three bases on which we will support work that people do. I've mentioned one. If it turns out to be a first class contribution to science, and I don't care what science it is, we will support it. But naturally, we have to use every technique at our command to make sure that it is good science. On the other hand, perhaps it won't be good science, but it may make an important contribution to the Ford Motor Company in that it invents, or develops, or from it comes, something important--then we will also support it. Or the person, by his work, by his personality, or by his interests, may interact with others and influence others and by so doing make it possible for the others to do some good for science or for the Ford Motor Company. That too, is a justification.

So here you have three reasons, three bases of evaluating whether the research is research that we want to continue to support. On this basis, we'll support it indefinitely.

Now, I'm taking too long, and I certainly don't want to encroach either on the time or the subject matter of the others, so I will close with one brief analogy--with an apology to Larry Bass who will probably be addressing his entire luncheon talk to this subject. It is a subject in which he is an expert, because he has spent many years at it, and I am a relative amateur. But I can't give up the podium here without at least alluding to this analogy.

We're concerned with what kinds of research we do and how do we do it at the Ford Motor Company, and why do we do it. We think we've been successful, at least the scientific community feels so, the Ford management feels so, and from the things that have gotten into the market place, I think even the stockholders now think so--and they are usually the last to recognize it.

What are some of the lessons that we can learn from others and what lessons can others learn from us? Well, when you are dealing with a company the size of the Ford Motor Company, something stands out immediately--if you saw our annual report, we did about nine and a half billion dollars

of business last year. Now that was an unusually high year. But one might bracket the sales of Ford in a given year between five and ten billion. This is roughly the same size as the gross business of the Air Force. It's also roughly the same size as the gross national product of the state of Pakistan. And certain analogies immediately emerge--let me leave out the Air Force for the moment; that may come up later in discussion after Bill Price's talk--but let me discuss the Pakistan situation.

Ford is very analogous to the state of Pakistan. You'll find that a few other states are roughly similar if you'll look at the table in Dr. Bass' new book that has just come out. He gives the gross national products and the amount of research in various underdeveloped countries--this is a "commercial" for you that you should go out and buy the book--published by Praeger and available at better book stores' everywhere!*

We, a multibillion company, while we make essentially one product, are diversified in making that product. So you might say we have certain natural resources within the Ford Motor Company. For example, we're pretty adept at making steel.

We're very adept at making glass also, and the same thing emerges here. We may not be as adept at making vinyls and polymers as Du Pont is, but we're still pretty good at it. So this is one of our natural resources.

Taking an overview of the future of the company and how science and technology relate to it we find that the thing to do is to try to strengthen our weaknesses, and even strengthen our strengths. We must not try to compete where we are not capable of competing. One of the fallacies of nations, as well as companies, is their enormous preoccupation with research and development in areas that they could not possibly usefully

* Lawrence W. Bass, The Management of Technical Programs: with Special Reference to the Needs of Developing Countries (New York: Praeger, 1965).

deploy, market, or compete in. High energy physics as a field for extensive research support makes no more sense for Pakistan than nuclear physics or steroid chemistry makes for Ford. Alternatively, there are undoubtedly areas of materials research utilizing some of the proven resources of the state which would protect their competitiveness in the world market where it would be wise for such a country to concentrate its research efforts. Thus, research and development planning is probably as important for a nation as it is for an industrial corporation.

III SCIENTISTS IN A TECHNOLOGY-ORIENTED ORGANIZATION-- THEIR EXPECTATIONS, INCENTIVES, AND CAREER PATTERNS

Donald G. Marquis

Dr. Vollmer (introduction):

Dr. Goldman mentioned that the kind of research we are talking about here is people-oriented research. It is research that lays great emphasis upon trying to set up an environment that is conducive to the utilization, the growth, the development of scientists, and the kinds of things that they are interested in in relation to the interests of their employer. Who are these people? What are they like? How are they like other men and how are they different from other men? This is the topic that we are going to discuss next.

Our next speaker received his doctor's degree in psychology at Yale University. Since then he has been a member of the faculty at Yale and also at the University of Michigan--becoming head of the Psychology Department in both institutions. He has also been active in governmental and scientific affairs as a consultant to several scientific advisory boards. He is past President of the American Psychological Association. He has done work in many different fields of psychology, and presently he is Professor in the Sloan School of Management at the Massachusetts Institute of Technology and is heading a continuing program of research studies on the management of science and technology. I'm pleased to present Professor Donald Marquis, whose topic is "Scientists in a Technology-oriented Organization--Their Expectations, Incentives, and Career Patterns."

Dr. Marquis:

One characteristic of scientists is that they are modest. The other two speakers have started with an apology, so let me say that when we got together a month ago to divide up the subject, I got stuck with one in which I am not an expert--namely, the characteristics of scientists. There are two people who are: Donald Pelz at the University of Michigan has been working for twelve years in this area. He was one of the very first to even think of it as a subject of research, and a summarizing book by Pelz and Andrews will appear probably about the end of this year. He is not available because he is in New Delhi this year. The other one is Howard Vollmer, who is not available because he is chairman of this session, but he is the one who should be giving this talk. I shall probably be misquoting some of his unpublished results and hope that he will correct me. We'll be talking, then, about people, which we've learned is an important component of research.

I will be making statements which sound like sweeping generalizations. Please take these as statements of average trends or statistical tendencies, recognizing that you can think of exceptions, and I can think of just as many as you, but it's important for planning research and for thinking about this subject that we recognize some of the general tendencies. I will not make any statements about differences or characteristics which are not statistically significant at the probability level of .05.

When we speak of scientists, who are we talking about? In order to keep it clean, let's admit that there are some scientists who are self-made. But the easiest way to define this subspecies of humanity is to speak of those who have taken a doctoral degree at a university in a field of science. There are about 80 thousand of them in this country. Of these, about 25 percent are primarily engaged in basic research, 15 percent in applied research, and a small number in development. About 25 percent are primarily engaged in teaching and 20 percent in management

or administration. Over half of the doctoral scientists are employed in universities, and about half of these say that they are engaged primarily in research. Thirty percent are employed in industry, and 4 percent in government. Since no set of these numbers adds to 100 percent we shall have to assume that the remainder are earning their living by some legitimate means.

This gives us some idea of the number of people, where they are working, and what they say they are doing.

Incidentally, I'll use the word "science" to refer to what Jack Goldman called "fundamental" or "basic" research and what Howard Reiss talked about as the "study of phenomena," and distinguish "science" from the rather ambiguous term, "research."

"Science" is what Ph.D. scientists do in a knowledge-oriented laboratory, as distinguished from a product-, process-, or commercial-oriented laboratory. There is a homily about managing a science laboratory to the effect that all you have to do is pick the best men and leaving them alone. Fortunately, not many people believe this, and there are two things wrong with it. First, there is no good method for picking the best men until they have repeatedly demonstrated their performance, and by that time they are settled, difficult to move, and expensive. The second reason is that if you leave them alone you will get much less productivity from them than if you pay proper attention to them, so let's get rid of the homily.

I first want to talk about what kind of people they are and what we can possibly do about selecting them. How do we recognize them? And second, because of what kind of people they are, I hope that other speakers will make clear what kind of an environment they need in order to be productive.

Now let me start out with a "black and white" difference between scientists and engineers--let's consider Ph.D.'s in science and bachelor's

and master's in engineering, because this sets up the polarity very clearly. Scientists and engineers differ recognizably by the time they enter college. They come from different kinds of homes, they have had a different kind of upbringing, and they behave differently in high school.

At MIT Benson Snyder and John Rule have been giving a two-hour battery of tests to entering students which includes measures of personality, values, attitudes, and interests. One very clear result is that freshmen, including those who do not know what major they're going to choose two years later, are different at the time they enter college. Those who major in a science field, as compared with those who major in an engineering subject, score higher, for example, in theoretical orientation, tolerance of ambiguity, esthetic interests, and desire for autonomy. Engineers, by contrast, score higher on desire for economic achievement and power, and on need for order and certainty. They are more socially extroverted and they engage in more organizational activities.

When they finish their education these differences between scientists and engineers have been maintained or intensified, so that at the time they are recruited for a job, they also differ in their desired job characteristics. Here the difference is very clear. It has been rediscovered in dozens of different research studies--many of them not substantial by themselves, but together they make a clear picture.

There are certain things in which scientists and engineers don't differ--they both want a job with high salary, with good facilities and resources for work, with security, they want to be treated as individuals, and they want to work in a good organization.

The scientist, however, emphasizes that he wants a job in which he will have freedom of choice in what he works on and freedom to follow up his own ideas wherever they lead him. He wants to make a contribution to knowledge. He wants an opportunity to keep up-to-date on new scientific developments in his field. He wants to publish. He wants to gain

respect in his scientific field, and he wants to be with expert, high caliber colleagues.

Compare that with the engineer's view. Typically, he says he wants a chance to move up in the organization, he wants a challenging job where he can solve practical problems, he wants an opportunity to see his ideas put to use. He wants to work on projects which will contribute to commercial enterprise, to welfare, economic growth, or defense of the country-- a lot of very important values. But they differ from those of scientists.

Of course, there are mixed types; there are scientists who have engineering values and there are engineers who have science values. But let me call the two types "science-oriented" and, because I can't get a better word, "commercial-oriented". The latter correspond with the values of managers, and engineers and managers are more similar than engineers and scientists in these respects.

Among scientists you will find variation--they are not all alike. Some of them will have commercial values, just as some of the engineers will have science values. In a recent study by John Hinrichs* at IPM, a questionnaire of 79 items, which had been carefully pretested was submitted to a national sample of chemists at the time they were finishing their doctoral work. The sample of 385 new chemistry Ph.D. graduates of 1961 included 41 universities.

Hinrichs found that there were three distinct patterns of attitudes. (The method was that of factor analysis for those of you who are statisticians; it's an objective method for finding which items cluster together.) The first pattern is what we have called science-oriented. He describes it as "reflecting attitudes valuing freedom and support in

* Hinrichs, John R. The Attitudes of Research Chemists. J. Appl. Psychol., 1964, 48, 287-293

research and a belief that industry raises barriers to worthwhile scientific activity."

The second cluster is what we have called commercial values. Let me tell you a few items that will help define these people. They tended to disagree with the statement, "most scientists are more interested in their profession than in an opportunity to move up in the organization for which they work." They agreed with the statements, "a chemist can put up with monotonous work if the pay is O.K.," and "in any organization, the people in power get there by manipulating other people." They disagreed with the statement, "a chemist must have freedom in applying his own ideas to solve technical problems if he is to produce significant research results."

Then there is a third category--a cluster of items such as "there is no conflict"; for example, "I can see the usefulness of science in a commercial organization and I don't see any conflict between them."

You would think that these chemists would get sorted into appropriate jobs. It turned out that of those who had taken jobs there were 152 who accepted academic jobs and 222 who accepted industrial jobs. But there was no difference in their scores on the three values. Thus the recruiting and self-selection process had not worked. I think that any of you who have had experience with recruiting know why--it's a pretty false process in which the promises of freedom and advancement are not always fulfilled. We'll get more evidence of that in a minute--that new recruits are very unhappy in the first few years on the job--whether it's academic or industrial.

Hinrichs carried out another study with an entirely different sample of employed chemists--286 Ph.D. chemists in three industrial labs. He plotted their scores on the three value components--scientific, commercial, and compatible--against the number of years they had been on the job--5, 10, 15, 20, 25.

Consider first the science value. It drops a little in the first few years and then shows no particular trend up or down. In general, those who have it hold to it.

The second component reflecting commercial values, which for these employed chemists is much higher, gets a little shock when they find that they aren't promoted in the first year; but later it goes up steadily. Those who have the compatible values of the third component show a gentle rise over time. This is the process of acculturation, socialization, or fitting into the organization.

Many of the results that have been published on some of these topics have presented a rather confusing picture when they describe the attitudes, values, and motives of employed research people. In the first place, they don't distinguish clearly between scientists, semi-scientists, and engineers. In the second place, they take a cross-section survey at some point when scientists have already become socialized, and so it's not surprising to hear statements as we heard yesterday--that scientists "love to work in industrial labs and contribute to new products and processes that will have commercial value." All right, that's true in that kind of a lab. The lab that was described yesterday is a mixed lab. It has some Ph.D.'s in it, but they don't behave like Ph.D.'s, we were told. By the end of the third year they didn't want freedom to choose their work and they didn't publish, even though the company encouraged it. So the company had already squeezed the science-oriented attitudes out of them.

Next we want to consider job satisfaction. A lot of work has been done on what makes research people satisfied--incentives, motives, job characteristics, and so on, and this is a very confused literature out of which I would like to draw a few verifiable statements. In general, the satisfaction of the individual in his job is important not for recruiting, because you can fool him on that, but for retention. The satisfaction of the individual is, in general, a result of how well the job and all of its

characteristics meet his expectations. We've seen that different kinds of people have different kinds of expectations and that they can even change their expectations and their attitudes over time.

For Ph.D. chemists, there is a positive and significant correlation between the third set of attitudes, which maintains that science and business are compatible, and satisfaction. People who have that kind of attitude (and the more of that attitude they have) are more satisfied on the job.

For the commercial attitudes, there is a negative correlation between the degree of the attitude and satisfaction. These people don't do very well economically. As you know, there is a pretty low ceiling on salary and promotion for researchers. They just got in the wrong business; they shouldn't have gone into research.

Similarly, those who are science-oriented and employed in one of these mixed labs, where the dominant atmosphere is set by those who accept the company goals and work toward new product and process improvements, also show a negative correlation--the more science attitude they have, the more unhappy they are on the job.

There is plenty of evidence that scientists and engineers are unhappy. Surveys have shown that they express more job dissatisfaction than other kinds of employed people. And we are beginning to get some clues as to why--they aren't fitted in right. It's not that they couldn't be satisfied, but it would take a different kind of organization or a different kind of job to do it.

Now let me turn to another topic--the factor of age. We have seen that there are changes in attitudes with age. There are also changes in productivity with age. You know the classic work of Lehman, published in 1953, in which he examined biographical information to determine the age at which scientists have made their major contributions. On the average, the likelihood of outstanding achievement increases to a peak in

the late 30's and early 40's, and thereafter it declines. The peak occurs earlier in the more abstract disciplines, like mathematics and theoretical physics, and later in the more empirically based disciplines--geology, chemistry, and biology. The other interesting finding is that for the most outstanding achievements, the peaking is sharper.

These are historical data. Surveys of current laboratories show that the highest productivity of Ph.D. scientists working in knowledge-oriented labs, measured by the number of publications or by the evaluation of their colleagues and superiors, is in the early 40's. Those working in applied and development laboratories, by contrast, reach their period of greatest productivity in the late 40's.*

Pelz found in addition that those who were high in their scientific values showed higher performance throughout their life and their peaks were not as exaggerated. This is confirmed in nation-wide studies of physiologists by Meltzer and others in which the highest productivity occurred in the late 30's, with another little spurt in the early 50's. For really good physiologists the peaks were sharp, for the average physiologists they were smoother, and for those in the lowest rank there was a small early peak before they disappeared from the published literature.

One last point: what's the difference between an outstanding and an average scientist? Disregarding the engineers, we will talk just about the top and the average scientists. There are three classes of factors to consider. First, there are characteristics of the people; you know that some are better than others. Second, there are some labs that are better than others; and these labs have certain characteristics which others will discuss on this program. Third, there is luck, or noise, or whatever you wish to label the large area of our scientific ignorance.

* Pelz, D. C., The "Creative Years" and the Research Environment.
IEEE Trans. Engineering Mgmt., 1964, 11, 23-29.

Among the factors that determine the level of individual scientific performance is the amount of education. As a matter of fact, the Ph.D.'s and those with master's degrees are two quite distinct subspecies in science. The master's are not significantly different from the bachelor's, but the Ph.D.'s are something else. They are not only finely screened, but motivation and educational experience result in a particular set of values and work habits. Among Ph.D.'s, intelligence test scores and course grades are not very discriminating, accounting for perhaps five percent of the variance in later performance on the job.

Another characteristic which has been extensively investigated is creativity, which is independent of brains or brightness. I won't take the time to go into the very confusing literature on this topic, but I can assure you that the evidence indicates that there is no test of creativity which, adequately validated on more than one sample, has shown a higher correlation with rated performance than .30, and these are results of my research. Such a test, therefore, could predict about ten percent of the variance in performance, independent of intellectual ability.

Differences in the degree of science orientation that we talked about earlier may account for about five percent of scientific productivity. Another factor studied by Pelz concerns dedication, or strength of motivation, or involvement, and showed a correlation of about .20 with productivity, and would account for about five percent of the differences in performance. These low correlations do not mean that the factors are not important--it's just that most everybody has them in sufficient degree. The differences in these factors may be small and therefore don't account for much of the variance in performance. I hope that point is clear. The age factor--which involves experience, senility, hormones, etc.,--accounts for about ten percent of the variance.

All together these factors might add up to 30 or 35 percent of the variance in productivity. They do not offer, therefore, any great hopes for improving scientific performance by better methods of selection of

personnel. The policies and procedures and the atmosphere of the lab-- the details of which I leave for others to discuss--are probably much more important than the differences in the people. I would recommend therefore that major attention in the management of science laboratories be given to providing challenging work, adequate resources, and discriminating recognition of excellence.

IV THE ORGANIZATION OF SCIENCE IN A TECHNOLOGY- ORIENTED ORGANIZATION

Howard Reiss

Dr. Vollmer (introduction):

Before lunch we heard about the rationale for having a science-oriented activity in a larger technology-oriented corporation. We've also heard about the kinds of people who need to be involved in this kind of activity in order to make it successful--the scientists. And now we want to talk about what to do with them, how to organize them, how to put them together, and what is the most effective way to do this.

Our speaker on this topic holds a bachelor's degree from New York University. He also studied at Princeton, Harvard, and the University of California. He received his doctorate in physical chemistry at Columbia University. He has taught chemistry at Boston University and has served as a chemist with the Eastman Corporation, with the Celanese Corporation, for some eight years with Bell Telephone Labs, and since 1960, he has been with the North American Aviation Corporation--first in its Atomics International Division for some three years, and since 1963, serving as Vice President of the Corporation and Director of the Science Center at North American Aviation, Inc. This is in Thousand Oaks in Southern California. And so I am pleased to introduce Dr. Howard Reiss, who will discuss "The Organization of Science in a Technology-Oriented Organization."

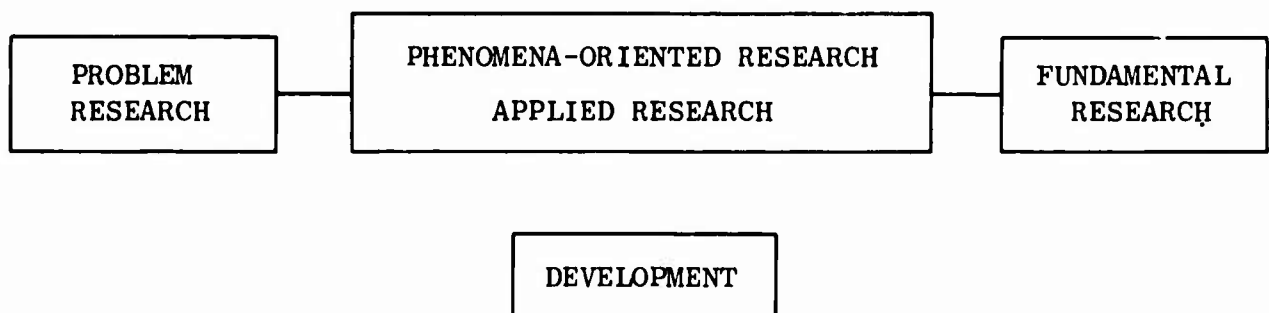
Dr. Reiss:

The previous speakers have set the stage very well for what I have to say. My subject is complicated by the fact that the organization of research in industry depends very much upon the kind of industry under consideration. Through force of habit, I will undoubtedly focus more on

my own kind of industry which happens to be the aerospace, or as we like to call it, the systems industry. Much of what I have to say, however, will have relevance for other kinds of industries.

The management of research is in fact an inexact science, and must therefore be approached from the descriptive rather than the quantitative point of view. As a result, it becomes very important to exercise care in advancing definitions. Even though definitions possess no absolute value, they do facilitate communication. If one deals with a descriptive science, like zoology for example, it is important to classify things in such a manner that the same understanding is enjoyed by all. For this reason, I will spend a certain amount of time talking about definitions. I feel strongly that the time has arrived in which the language of research management should be more uniform.

May I have the first slide?



Looking at this slide, I wish to make a distinction between two kinds of activities, namely "development" and what I call "research". This distinction is absolutely necessary for clear thinking in research management. Research and development are very different functions. Under "research in industry" it is possible to identify two fairly distinct kinds of research groups. One of these I will call the "phenomena-oriented research group" and the other the "applied research group". I intend to elaborate on these subjects shortly. Both of these research groups can perform two kinds of research--"problem research" and

"fundamental research". Notice that the terms "applied research" and "fundamental research" are not used as opposites--the applied research group can be doing fundamental research.

Development is something else. In any one company it will most likely be the larger of the activities identified in the slide, but we place it in a small box here because I wish to concentrate on both phenomena-oriented and applied research.

May I have the next slide, please?

PHENOMENA-ORIENTED RESEARCH

Replication in DNA
Thermodynamic Properties of Liquids
Superconductivity

Jack Goldman treated the subject of motivation very nicely. That is, what are the reasons for having these kinds of research groups in industry? It is not my intention to argue these points. My assignment concerns the method of managing these functions once it has been determined that they are of value to a technological organization.

I should, however, make one comment about the role of the phenomena-oriented research group because I do not think that Jack mentioned it, probably because it has more relevance for a systems-oriented organization like my own. This is the role that the phenomena-oriented group may play in colonizing the company's scientific interest in the external community of science. Let me give a couple of examples. The first of these has to do with the transistor. When the Bell System discovered the transistor effect (the so-called point contact diode) it knew immediately that this effect could be translated into a device which could be used to replace the millions upon millions of vacuum tubes then

existing within the Bell System. Now these tubes consumed power, possessed finite lifetimes, and used up space. The Bell System didn't really care to sell the transistor, it only wanted to use it. It was a systems organization.

It emerged that some very difficult problems in solid state physics would have to be solved before the effect--for it was only an effect then--could be converted into a reliable device. Rather than perform all of the research necessary in this respect by themselves, Bell used tradition and well-developed motivational apparatus to induce some of their very best scientists to work in the field of solid state physics in this fundamental way. These scientists stimulated the external community by their publications to such a degree that a physicist in the 1950's went to the Physical Society meetings and found that physicists were working essentially on two things: either particle physics or solid state physics. As a matter of fact, in some of the solid state physics sessions there was standing room only--the sessions had to be played twice. There was tremendous amplification and feedback. It seemed as though the entire world was helping Bell in its effort. Bell's research proved to be very sound business strategy. As a matter of fact, partly due to the so-called "consent decree" but not entirely, Bell also licensed about 120 firms to help in the development of the transistor--because they wanted to use it, not sell it. So it colonized its interest on two fronts: in the world of science and in the world of industry.

In our company we have a similar situation which may serve as another example. One of our divisions is one of the large producers of nuclear reactors in the country. At the moment, the nuclear reactor market is marginal, and some of the major problems which militate against the economic application of reactors are once again solid state physics problems. The so-called fuel swelling problem is a case in point. At present, one uses up about one or two percent of the uranium fuel elements and is then forced to remove the elements and reprocess them.

Anything which can be done in the field of so-called "atom movements in solids" which will hasten the day when some of these material problems are solved so that the nuclear power market becomes less marginal helps our company. It helps even if our competitors get a share of the market--as long as we get our fair share--because it's better to have a fraction of a finite market than all of a nothing market.

The point is that a role does exist in industry for a research group which can perform subtle functions such as those which Jack Goldman mentioned and the one I have just described. The intensity of the need depends, however, upon the particular company in question. A consumer products company might not find the same use for the "colonization of interests" function. It might even tend to restrict rather than enhance the flow of scientific information. On the other hand, a systems company may, as we have seen, have several sound business reasons for enhancing the flow.

With this one addition to Jack's list, let's get on to our definitions. Referring to the second slide, we see here some examples of phenomena-oriented research. Consider the study of the replication process in genetic material--the DNA molecule for example. One might be interested simply in the mechanism by means of which the molecule comes apart, how it reproduces itself, and so on. This is the study of phenomena, and in a sense it names itself. Or we might examine the thermodynamic properties of liquids for the purpose of understanding the relationship, let's say, between heat capacity and heat of vaporization, or the expansivity, or the surface tension of liquids in terms of the behaviors of individual molecules and the forces between them, and so forth. All of this is phenomena-oriented research. The end purpose involves the understanding of phenomena. Superconductivity, which was mentioned by Jack, is clearly a phenomenon (a macroscopic quantum mechanical phenomenon, highly nonclassical). Arriving at an understanding of superconductivity is phenomena-oriented research.

Now may I have the next slide?

APPLIED RESEARCH

Nuclear Gyroscope
Transistor
Synthetic Rubber
Nuclear Power
Xerography
Spinning of Rayon
Aging of Wine
Stainless Steel

Now what would an applied research group do? Here are some examples of applied research. The nuclear gyroscope uses the spinning nucleus of an atom as a gyroscopic element. The major problems associated with the development of a nuclear spin gyroscope do not involve normal mechanical gyroscopy but nuclear spin relaxation times--that is, how long does it take a spinning nucleus once aligned to misalign? Information is stored in the original alignment. Or the development of the transistor--that's clearly applied research. The end purpose involves an application. The development of a technological artifact.

Synthetic rubber--one would have to do a great deal of fundamental research here. These examples serve to indicate that the terms "fundamental" and "applied" when applied to the description of research are not necessarily opposite. In the example of the nuclear gyroscope, the study of nuclear spin relaxation times could never have been pursued by Mr. Edison employing his purely empirical approach. What we mean when we say "fundamental research" is that the scientist thinks of himself as studying fundamental mechanisms--knowing a great deal about such mechanisms. "Fundamental research" refers to the method not the end. The term "applied research" refers to the goal or end purpose. In the same

way, the term phenomena-oriented research refers to the end purpose. The purpose is to achieve understanding of phenomena. Both groups may do fundamental research.

The conventional use of the term "fundamental" in the description of research involves applying it to what we have been calling phenomena-oriented research; but both the phenomena-oriented and the applied groups can do fundamental research, and I think it is very important to make this clear.

Synthetic rubber may be considered in the same way. One is forced to acquire a great deal of understanding concerning the bonds between carbon atoms and a polymer molecule--how they can be produced, and so forth.

Similarly with nuclear power and xerography. These two, by the way, are examples of applied research involving a process rather than an artifact as an end purpose. In both cases, however, they are applied research because one seeks understanding which leads to an application. In fact, the key which defines research (as distinct from development, for example) is the fact that one seeks understanding.

May I have the next slide?

<p style="text-align: center;"><u>PROBLEM RESEARCH</u> Heat Treatment of Silicon Monodispersed Aerosols Storage of Liquid Hydrogen in Rockets</p>

Both kinds of research groups, but probably the phenomena-oriented group more than the applied group, perform what I call "problem research". That is, they may seek understanding which may help a development group solve a "problem". The slide has three examples. The heat treatment of

silicon is one. When the transistor was already a well established fact, it emerged that if one grew silicon crystals from the melt and then rotated the crystal as it was pulled from the melt, one would obtain a crystal with unexpected (and detrimental) electrical properties. Such properties were not associated with crystals which were drawn from the melt without rotation. For the rotated crystal, it turned out that when the crystal was heated to a temperature of 400° , its electrical properties underwent a mysterious change. This did not happen with crystals which had not been rotated.

At Bell, the fundamental research group, or what I would prefer to call the phenomena-oriented group, was assigned this problem--not directly but by more subtle motivational means. A very detailed study of the process was carried out, and it was discovered that oxygen atoms were being stirred into the rotated crystal. At temperatures about 400° , the oxygen in the crystal managed to move in a detailed and special manner so that the electrical properties were changed. In this example, detailed atomistic understanding of the mechanism of change was acquired, but the problem arose in connection with a development program associated with an established device.

Then there are monodispersed aerosols. During the second world war, problems arose in stabilizing insecticides which had been dispersed in aerosol form so that the individual aerosol particles did not grow and settle too rapidly. It was discovered, on the basis of a mechanistic analysis, that aerosols containing broad distribution of particle sizes were inherently less stable than those in which there was essentially one particle size. Such aerosols are called monodispersed aerosols, and their introduction contributed to the control of diseases like malaria in the South Pacific. Again, we have an example of problem research in which an understanding of fundamental mechanisms was required.

I won't consume the remaining time talking about the storage of liquid hydrogen in rockets. This is another example in which fundamental

understanding is required by people engaged in a development program. In our company, we happen to be thinking about this problem right now. Such matters as the effects on storage of ortho- and para-hydrogen, and other quantum-mechanical phenomenon are involved.

May I have the next slide?

DEVELOPMENT

Corrosion Resistance of Tin Cans
Durability of Paint
Wearability of Carpets
Response of Electronic Systems to Radiation & Shock
Assembly of a New Computer
Elimination of Rocket Engine Instability
X-15 or XB-70 Research Aircraft

Here are some examples of development. If one pours prune juice into a tin can and observes how much time passes before the can corrodes without seeking any understanding--that's development. More specifically, that's "testing"--the simplest kind of development. Or if paint panels are exposed to the sun for the purpose of determining how the paint wears--this is development. Or if the wearability of carpets is determined by having people walk on them--this is testing also. Of course, the data collected in this manner are useful even if understanding is not involved. There are much more sophisticated examples of development. One may take electronic systems and expose them to shock or radiation to determine how they respond to these environments without necessarily having acquired any sort of detailed understanding. This is development. The assembly of a new computer can be a very sophisticated undertaking, but nevertheless it would be defined as development. Here the creative act is associated with the synthesis which occurs through the integration of existing knowledge components. This is another characteristic

which identifies an activity as development--the fact that the novelty resides in the act of synthesis.

The so-called X-15 and XB-70 research aircraft aren't really research aircraft. They represent development programs, because most everything going into them builds upon existing knowledge. I don't mean to imply that the activity is not both creative and important. Only that it differs from research. Or consider the elimination of instabilities in rocket engines. Engineers change configurations and add baffles until such instabilities are gone. Frequently, they are satisfied if they can solve the problem without understanding the detailed process by means of which the solution was obtained. This sort of thing is development.

Now, in this talk I do not expect to discuss the organization of a development program. This is an important topic, but one for which I have neither the time nor purpose here.

May I have the next slide, please?

CLASS OF TECHNOLOGY-ORIENTED ORGANIZATION

Consumer Goods
Systems
Government

I mentioned that there are several kinds of technology-oriented organizations. We've heard about three of them today--consumer products organizations, systems organizations, and government agencies. These are characterized by companies like Ford and North American Aviation, and technical agencies such as the one which Bill Price will discuss. Each is different. A systems organization does not usually mass produce its systems. A system may include a very complicated chain of technological components, each very sophisticated in itself. By the same token, a

multi million dollar system may depend critically upon a three dollar component. As a result, it is just as necessary to be concerned about research in connection with the three dollar component as with respect to other more expensive components.

By contrast, in a consumer products organization the amount of research performed in a given area is somehow related to the amount of business in that area--how many of the products are mass produced, and so forth. One can see right away that different kinds of considerations have to be brought to bear upon research in this situation.

Now may I have the next slide, please?

CHARACTERISTICS OF ENGINEERING ORGANIZATION

- 1) Large
- 2) Management makes technical decisions
- 3) Hierarchy for communication
- 4) Problems are cost and time
- 5) Planning makes long lead times possible

Here I wish to discuss the characteristics of an engineering development organization as opposed to those of a research organization. The functions are different. In this connection, I should mention that a serious problem arises because the statement that the function of one group is different from that of another, and that therefore the two groups must be treated differently, is sometimes misconstrued to mean that one group is more important than another. That's not the point at all. The functions are different; and therefore the groups do have to be dealt with in different manners, and management must be sensitive to these different functions.

What are the characteristics of an engineering organization? First, it is usually quite large. Management often makes very detailed technical

decisions in an engineering organization. As a result, the management structure must be pyramidal, so that information can be channeled through node points at every supervisory level on up into the hands of a few people at the top who really do in fact make important technical decisions. Also, the major problems are usually those of cost and time. That is to say that one generally knows in engineering development whether or not something is ultimately feasible because one works, as I mentioned earlier, with existing knowledge components. There are exceptions to this rule, but in general it is how long will it take and how much will it cost that are the important questions. Because one can perform things like PERT analyses from which critical paths can be selected, it is possible to arrange for long lead times in the procurement of men, materials, services, and so on.

In contrast, it is very difficult to do these sorts of things in research organizations, because--if you will let me have the next slide please--

CHARACTERISTICS OF RESEARCH ORGANIZATION

- 1) Small
- 2) Management creates proper environment and coupling
- 3) More horizontal organization
- 4) High cost on a per man basis
- 5) Problems involve feasibility
- 6) Services must be flexible to capitalize on breakthroughs

--research organizations have quite different characteristics. Usually they are pretty small compared to an engineering organization, especially if a phenomena-oriented group is involved. The principal function of

management in a phenomena-oriented organization is not, and certainly should not be, the rendering of detailed technical decisions. Experts are employed for this purpose. This doesn't mean that the manager doesn't have control, nor does it mean that he should not think about technical matters; but the control should be related, first of all, to the establishment of an environment in which creative activity can flourish. Beyond this there is the enormously important problem of coupling a research organization to the rest of the company. That's a full time management job in itself.*

Research management should not worry about detailed technical problems--that's the work of a scientist. That's why he was hired in the first place. If a manager has to concern himself with detailed technical work, there is no point in recruiting a scientist. One might just as well have hired a high-class technician.

A research organization should be more horizontal than a development organization. Research is generally high-cost, and you don't want to have 20 Ph.D. scientists working on things which are the reflection of the ideas of one man. What is desired is the greatest reflection of ideas from the greatest number of people, because research is so high-cost on a per-man basis. Thus a more horizontal organization is suitable. Again, this doesn't mean entirely horizontal, because one cannot countenance anarchy either, but the organization should be more horizontal than an engineering one.

In a research organization emphasis should be placed more on professional eminence than upon rank and title--the more conventional status indicators. I think this confirms what Dr. Marquis said earlier, that the desires of scientists are really different on the whole than

* See Appendix A for data on the role of research management in "coupling."

those of engineers. The engineer looks for climbing up a vertical promotion ladder. The scientist is more concerned with other things.

In research, feasibility is a major problem in contrast to the situation in engineering--wherein I mentioned earlier cost and time are major problems. One wishes to take advantage of a scientific breakthrough, and it is necessary to have a small flexible organization fast on its feet in which six months are not required for the acquisition of a set of complicated equipment. The services that support the technical organization have to be different. The research organization must be able to capitalize on breakthroughs.

This is the sort of organization which I shall now consider. May I have the next slide please?

DEALING WITH THE PHENOMENA-ORIENTED GROUP

- 1) Mission to remain expert but to couple
- 2) Secular immaturity of young scientist
must be reckoned with (reacculturation)
- 3) Control in a permissive atmosphere
(choice of man)
- 4) Motivation in a permissive atmosphere
 - a) scientist managers
 - b) task force technique
- 5) Maintain the respect of scientific
community

There is a great deal of nonclassical science in industry today. I mean nonclassical in the scientific-technical meaning of the word--for example, quantum mechanics, relativity, and so on--things which are outside of the pale of our everyday experience, and therefore outside the region in which our intuition is trained. The manager must accept the fact that one of the roles of a phenomena-oriented research

group is to remain at the scientific frontier. Therefore, a great deal of research which he performs is simply aimed at the maintenance of expertness, not problem solving. Of course, if the members of the phenomena-oriented research group remain expert and do nothing else, they are of no value to the company. But one has to face the fact that the group has to remain expert. I dislike using the word "freedom" in describing what seem like privileges granted to the members of a phenomena-oriented group. There has been much confusion here. If, say, a fellow interacts 20 or 30% of his time in some direct manner with the applied science and development activity of the company, then one often thinks that he should be rewarded by allowing him 70 or 80% of his time to do what has been called "free research". Herein lies the confusion. One should not regard this so-called freedom as a reward, for it is incumbent upon the man in question, and should be regarded so by his company, to remain expert in the same manner that a professor remains expert by performing research at the frontiers--the better to be able to instruct his students.

The precise manner in which a member of staff in a phenomena-oriented group divides his time between these two kinds of activity depends upon the individual company. Management must play this one by ear, but one can be sure of this: unless management regards it as one of its functions to control the balance between the activities of interacting with the company and remaining expert, the function of remaining expert will eventually be lost; and that is too bad, because function should not have been instituted in the first place if it was so unimportant as to be expendable. You don't want to lose the nature of the research group, so it becomes management's job to make certain that a member of staff has time to retain his expertness, and that he is told at the outset that this is part of the job.

One does have the question of what I like to call the "secular immaturity" of young scientists, because that is what it is. It has to

be reckoned with, and we heard something about this today. The average scientist emerging from the university, if he is in the upper 10% let's say of new Ph.D.'s, will divide the world into heroes and villains--two classes. The heroes support science and the villains don't. It wouldn't be so bad if he arrived at this conclusion because of a belief that science benefits humanity; but closer inspection reveals that this has nothing to do with it, and again I'm speaking about the average. The attitude is a totally religious one--science for science's sake and nothing else. Now we have to accept this for what it is. One doesn't take a religious fanatic and hit him over the head saying, "Now be different." The fact is, under these circumstances, he would rather be hit over the head--he would rather be a martyr. So one has to deal with this in a sensible way. We can apply the term which was used before--"reacculturation". The sort of scientific laboratory about which I am talking is an ideal place in which a wise management can use reacculturation to turn all of this scientific energy loose, properly channeled of course. of course.

It is difficult to control a program in a very permissive atmosphere; but the method of control does not involve going to the scientist and saying, "Do this," and three weeks later saying, "Now, you do this." Control must be before the fact, before the man is hired. One certainly doesn't recruit a man into a phenomena-oriented group who has no interest in identifying with industry. You endeavor to hire the blended type of fellow about which Dr. Marquis spoke. Generally he can be identified. Our method for identifying them is as follows: We attempt to follow a young man through his graduate career, especially during the last one or two years. We read his publications and we know he is a promising scientist. We speak to the professor; and finally the man visits our laboratory for an interview. He may spend two or three days, and present a scientific seminar. He talks science with our people and so on. During one of these days I will make time to conduct him on a tour around our company. I'll make certain that he sees some

systems work first hand. For example, I would arrange to have him see the Minuteman computer assembly line; or stare at a nuclear reactor. I'll let him hear a rocket engine fired, and perhaps have him see the XB-70 or the Apollo spacecraft mockup. Usually, a new Ph.D. scientist will not be familiar with these kinds of things. Engineers may be, but not young scientists. I watch him; and if he shows no spark of enthusiasm upon viewing this fantastic technology, then I know that we are asking for trouble if we recruit him no matter how good he may be as a scientist. In our permissive atmosphere he would simply hasten to withdraw as much as possible from the company. We try to screen these fellows very carefully at the outset.

Our program is controlled through the choice of men. If we wish to explore a particular area of science, we choose a man whose natural inclinations are to work in that field. Once he is on board, however, he is the expert; and he determines what problem he'll work on so as to remain expert. But at the outset he knows that he is expected somehow to couple with the rest of the company. In this connection I agree with something Dr. Marquis said; namely, that we have a terrible system of false recruitment in this country, and it makes for a lot of unhappy people. It is not fair to a student, and it doesn't help the company either. One has to spell it out right at the beginning--the fact that coupling with the company is required. There is nothing to prevent one from indicating his pride in the scientific record of his company, but at the same time it must be emphasized that a balance between professional interest and overall company purpose must be achieved.

How can this be done? I submit that in a phenomena-oriented group the only way of controlling motivation is to have scientist-managers; that is, people who have really been through the mill, who may even still be doing scientific work, and who the young people can respect. Reacculturation, you see, cannot be accomplished by a member of what is patently the enemy camp. It has to be done by a fellow with whom the

intersection of the scientific and business cultures exists. These men are few and far between, but they do exist.

If a problem arises, for example, in which our research group can help, I or one of our associate directors will form a little task force. For example, at one time I crawled through the tanks of the XB-70 fixing leaks. I'm a theoretical chemist; but all of this was part of the motivation program for younger scientists, some of whom also crawled through the tanks with the same purpose in mind. Such young fellows will say, "Well, here's a man who has a pretty good scientific reputation; he doesn't experience any pains of guilt, nor is he betraying his colleagues by doing this." There is a real question of getting over the guilt syndrome associated with doing this kind of applied work.

In connection with the fuel leaks occurring in the XB-70 case, it was a high temperature fused salt chemist, one of the best in the country as a matter of fact, who discovered a neat way of detecting these very small leaks. This didn't solve the problem totally, but it helped quite a bit and he was very happy about it. As a matter of fact, he filed a patent on the device later. In the end he returned to his phenomena-oriented work because that was his primary job, but nevertheless his intelligence served us well when it was needed.

With a phenomena-oriented group it is important to maintain the respect of the scientific community, so that one can recruit outstanding people and also facilitate communication with that community. We like our scientists to visit universities and be treated not as members of industry but as members of the scientific profession, so that there is an easy and rapid exchange of information--so that we learn of scientific developments as early as possible. This can only be accomplished through maintaining the respect of the scientific community. This means that publication must be considered an important activity for members of staff. The exchange of information is a two-way process.

May I have the next slide?

DEALING WITH THE APPLIED GROUP

- 1) Mission-oriented to applications
- 2) Attenuation of scientific ardor must be minimized (good fundamental scientists)
- 3) Control is more direct--but not entirely (choice of man again)
- 4) As much as possible--must eliminate politically-oriented managers (managers must encourage coupling)
- 5) Maintain respect of scientific community

I don't intend to say as much about the applied group as I have in connection with the phenomena-oriented group. For one thing the role of the applied group is better--even if not perfectly understood--than is the role of the phenomena-oriented group. It should be emphasized that in an applied group it is very important not to attenuate the scientific ardor of the scientist. If the emphasis is always on the application and not on the science behind it, then one soon loses adequacy of function. Applied groups should be populated by good fundamental scientists--that is, people who know fundamentals, who are interested in fundamentals, who will go to scientific meetings, and so forth. William Shockley, for example, who was very instrumental in developing the transistor, was an outstanding fundamental scientist; but he had a burning enthusiasm for applied science and his major interests were, and are, centered in this field.

In the applied group control is more direct, but not entirely direct. Again, it depends upon the choice of men. One shouldn't employ people in these groups who aren't interested in application, whose enthusiasm is limited at the outset. Otherwise, the result will be an

unhappy person endeavoring to do phenomena-oriented work in an applied science group. Actually, you can often get these kinds of people from the phenomena-oriented group, some of whose members of staff eventually become interested in application; and at that time it is natural to transfer them.

Now this item is very important. As much as possible, one has to eliminate politically-oriented managers. Managers must not compete with each other--functions have to be sequenced. One cannot have a manager of an applied group competing with a manager of a phenomena-oriented group, of a development group, or some other engineering group, in such a manner that he obstructs the flow of information. Frequently such competition and obstructive behavior is motivated by a desire for advancement. One has to have fellows who are really interested in the systems approach of the entire organization, that is, insofar as their function is concerned. If these kinds of people are not selected--trouble lies ahead. As a matter of fact, a good strong manager whose heart is in the right place, or to put it another way, whose heart is pure, if he sees one of his subordinates behaving in such a strong politically-oriented manner should take steps--discrete steps--to eliminate him from the organization. In the end he won't do it any good; and again, it is important to maintain the respect of the scientific community.

May I have the next slide, please?

- 1) If possible, same management should be circulated through both the phenomena-oriented and applied groups.
- 2) The groups should not be managed by engineers who frequently do not understand the research function--simply because it is different from the engineering function.

If it's possible, the same management should be circulated through both the phenomena-oriented and applied groups. We are beginning to do some of this in our company now.

I have mentioned this before--research groups should not be managed by engineers who frequently do not understand the research function. This is not to imply that engineers are neither as intelligent or as valuable as our scientists. Frequently their work is more valuable but nonetheless different. Their outlooks are different, and their function is different. Scientists should be managing research groups.

The next slide has one statement:

The best way to facilitate coupling and proper use of the research function is through the slow process of moving the proper research people into management.

The best way to facilitate coupling and the proper use of the research function is only through the slow process of moving the proper research people into management. This takes time, and one has to be patient. One must develop a cadre of sympathetic people throughout the company who understand both research and company problems.

V WORK UNIT EFFECTIVENESS IN A SCIENTIFIC ORGANIZATION

Floyd Mann

Dr. Vollmer (introduction):

Suppose we set up this kind of a fundamental research activity within a larger organization. How do we know what we are getting out of it? How can we measure the effectiveness of this kind of a program in view of the fact that these kinds of research programs do not produce tangible items, devices, but they produce ideas -- things that are rather intangible?

The next speaker is going to cover some of these points on how you can measure the effectiveness of research groups and what might be done to enhance the effectiveness of research groups. He received his bachelor's and master's degrees at the University of Iowa and his doctorate in the field of sociology at the University of Michigan. He has served as an economist and statistician in the Bureau of Labor Statistics, and also in cost-of-living research there. He has served as a study director at the Survey Research Center at the University of Michigan since 1947, and also as a program director or assistant program director at the same organization. Since 1953 he has been a professor of psychology at the University of Michigan. From 1963 to the present he has been the Director of the Center for Research on the Utilization of Scientific Knowledge at the University of Michigan. I am happy to present Professor Floyd Mann.

Dr. Mann (summary of contents):

The principal objective of this study was to identify the social and psychological factors which differentiated more effective from less effective sections of scientists in one of the research bureaus within a large governmental department.* Two sections were randomly selected from each of fifteen divisions in the agency. The sections were ranked by five key members of management on five dimensions of effectiveness: productivity, efficiency, adaptability, cooperativeness, and staff recruitment and development. Questionnaire data were obtained from the section head and five randomly selected non-supervisory scientists in each section. The questionnaires contained questions concerning individual characteristics -- demographic factors, skills, satisfactions, health complaints; occupational information -- valued attributes of a job, career plans, reference groups; supervision and management -- perceptions of supervisor's skills and behavior; and organizational characteristics -- mission, coordination, adaptability, distribution of influence, tension. The rankings of the five key managers proved to be significantly related to each other on each dimension. The rankings of different dimensions were found to be rather highly related. All of the

* A full account of the findings from this study will be published as a monograph. Franklin W. Neff, John C. Erfurt, and Floyd Mann have shared in the direction of this study and analysis of the data. The research was supported by the Behavioral Science Division of the Air Force Office of Scientific Research (AF 49 (638) 1255), and the departments in which the study was made.

rankings were therefore combined into a single measure of overall effectiveness. On the basis of this single measure, the sections were grouped into high, medium, and low effectiveness, using breaks at quartile points.

The meaningfulness of this criterion measure of section effectiveness obtained from key management was explored by (1) constructing similar overall unit effectiveness measures for section chiefs and scientists from questionnaire items that approximated the five dimensions used in the rankings, and (2) interrelating these three overall indices of effectiveness. The interrelationships among these three independently obtained sets of data from personnel at three levels in the organization were statistically significant at the .05 level or higher. This high degree of interrelationship indicated a "sharedness" among management, section heads, and non-supervisory scientists about the organization's standards of overall unit performance and how well each unit was doing in meeting these standards. With this "sharedness" to support the validity of the information obtained from top management, the questionnaire data from first the section heads and then the scientists were studied to learn what factors distinguished high, medium, and low effectiveness sections. The findings regarding the section heads only are reported.

More supervisors in the high effectiveness sections (as opposed to those in the less effective sections):

- are younger
- have more education
- have higher civil service job grades

-- are similar in age and educational achievement with the scientists in their sections

-- consider their occupation more important in their lives than their family

-- are more likely to prefer work situations which afford greater chances to exercise authority and achieve success at the risk of security than their counterparts

-- view periods of major change as more exciting than annoying, and providing an opportunity to use their abilities

-- perceive their own career in terms of a profession or speciality rather than the public service

-- identify their professional colleagues in their part of the agency as their most important reference group

-- tend to attach high importance to these work goals that are characteristic of the scientific professional

-- see research and development efforts as being crucial to their agency's basic mission, and indicate that the proportion of the agency's total resources devoted to research is either adequate or that a greater proportion should be devoted to research

-- report high levels of opportunity on their jobs to attain (1) their scientific goals, (2) their socio-emotional goals of having congenial co-workers, of being evaluated fairly, and of having stability of employment, and (3) their public service goals

-- report high overall job satisfaction

-- are satisfied with their opportunities for technical and administrative training

-- say their occupation is the most important sector of their lives, and are satisfied with their performance in this sector

-- say they worry more often about money problems, about how good a job they are doing, and about feeling "in a rut"

-- report fewer mental health complaints and say they never worry about their own health

-- rate their immediate superiors very high or high on (1) using supportive behaviors such as getting their ideas and suggestions, giving them help when they really need it, making it free for them to discuss job problems with him, being open to influence to a considerable extent, using general rather than close supervision, and being good at human relations, (2) on coordinating and integrating activities: being up-to-date on new policies and procedures, planning work so that time is not lost, assigning work so that there is no duplication of work assignments, doing administrative activities well, and giving little attention to enforcing rules and regulations

-- report reciprocal high understanding between themselves and their subordinates in the unit.

This is a partial list of the findings about how the supervisors in the high effectiveness sections differed from those in the low effectiveness sections. Many but not all of these findings appear to hold for the

non-supervisory scientists in these same sections. Further analyses will be required before the full story is in, but this study, using the administrative unit of the section as the unit of analysis, suggests that many of the factors that have been found over the years to distinguish high and low effectiveness units in other types of organizations may hold for scientists working in groups.

VI THE AIR FORCE OFFICE OF SCIENTIFIC RESEARCH AS AN AIR FORCE
ACTIVITY TO UTILIZE THE EXTRAMURAL SCIENCE-ORIENTED COMMUNITY

William J. Price

Dr. Vollmer (introduction):

Some people still maintain that it is simply a matter of opinion how you should organize research activities, or how you decide which forms of research organizations are the most effective, and I think we all agree that there is still room for differences of opinion in many aspects of how you organize research. At the same time, however, I think the last presentation begins to indicate the vast amount of data that are now being collected relevant to this subject, so that we are beginning, not only in the study reported by Dr. Mann, but in a lot of other research efforts, to build up a body of data which all say essentially the same things about the most effective ways to organize, to administer and to lead the kinds of research organizations that we have been talking about here.

Now I would like to introduce the individual who was actually the designer of this panel, who had the concept for it in the beginning, and who set the panel up. He has a bachelor's degree from Denison University, a master's degree and also a doctor's degree in physics from Rensselaer Polytechnic Institute. He has done teaching and research at Rensselaer, and he has been a research engineer in the Bendix Aviation Corporation, a research physicist at Battelle Institute, and a professor of physics and later the head of the Department of Physics at the Air Force Institute of Technology, located at Wright Field, Dayton, Ohio. He has also served as chief scientist at the Aerospace Research Laboratories of the Air Force, also at Wright Field, and which is, incidentally, one of the fundamental research laboratories of the kind that I think we have been talking about

in industry. Our speaker played a prominent role in making that organization the kind of organization that it is today. Since 1963 he has served as Executive Director of the Air Force Office of Scientific Research, which is located within the Office of Aerospace Research, the fundamental research activity in the Air Force. His topic today is "The Air Force Office of Scientific Research as an Air Force Activity To Utilize the Extramural Science-Oriented Community."

Dr. Price:

After listening to the very fine presentations which have preceded, I find that the subject of the series has been covered very well from a general standpoint; therefore, I will restrict my remarks to the description of a particular science-oriented activity which falls into this general class, namely, AFOSR, the organization which I know best.

AFOSR is a part of the Office of Aerospace Research, the latter being the Air Force agency responsible for fundamental research in the Air Force. Thus, OAR serves a role for the Air Force, analogous to that served by the North American Aviation Science Center for the North American Aviation Corporation and by the Ford Research Laboratory for the Ford Motor Company.

The AFOSR research program is accomplished by contracts and grants throughout the U.S.A. and a number of other countries. OAR also has science-oriented in-house laboratories, namely, Aerospace Research Laboratories (Dayton, Ohio), the Air Force Cambridge Research Laboratories (Bedford, Massachusetts), and the Frank J. Seiler Laboratory (AF Academy, Colorado). I will not be discussing the programs of these latter laboratories.

What I am talking about are the contracts and grants which are funded by about two-tenths of one percent of the Air Force budget. As is the case with all other government agencies which depend on technology, a

certain fraction of the Air Force budget goes into support of basic science, characterized by that carried out in universities. In our case, about 80 percent of the monies which I am talking about are spent in universities; the rest in nonprofit R&D organizations and industry.

I should point out that the subject of this session is very timely in light of some hearings which were in progress in Congress this week. These hearings are on the subject covered by the report entitled, "Basic Research and National Goals," prepared by the National Academy of Sciences for the House Committee on Science and Astronautics.

The NAS Committee was asked two questions by the Congressional Committee:

I. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?

II. What judgment can be reached on the balance of support now being given by the Federal government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?

In responding to the question concerning the allocation of resources between science and other activities, the NAS Committee found it desirable to categorize basic science in several different ways--in addition to subject matter fields. Some of the categories enumerated were: motivation for the scientific research accomplished--as culture, as an adjunct to education, as a means to accomplish nonscientific goals; the nature of the performer--whether university, government lab, or private industry; the "character"--whether "little science" or "big science." But the category which interested me most was the source of support--whether a science-oriented agency (the National Science Foundation) or mission-oriented

agency (all others). It is my hope that this session here today has contributed to the understanding of the role and importance of basic science for the mission-oriented agency or organization.

The overall purpose of AFOSR is to help assure that scientific research activities have a timely impact on the Air Force, particularly the future operational Air Force. We engage in this mission, recognizing full well that we exist along with a very broad spectrum of research, development, testing, and engineering accomplished by other mission elements of the Air Force. Consequently, our portion of responsibility is clearly science-oriented in order to help exploit the research end of the spectrum for Air Force purposes.

It is a matter of historical record that under wartime situations research scientists may be mobilized to work very effectively toward the defense of the country and that their activities have a major impact toward this end. World War II has many outstanding examples of this.

During peace time or even in limited war situations such as now, the same or similar groups of scientists can still have a very important influence on the defense stature. However, for many obvious reasons which I will not attempt to enumerate, it is clear that the current methods of involvement of science-oriented people in the Defense Department business must be quite different in many respects than in the case of wartime mobilization. AFOSR serves the Air Force by providing various effective mechanisms by which highly creative science-oriented personnel contribute to the defense stature and at the same time pursue uninhibitedly their chosen science goals.

At this point I want to clearly designate the group of scientists about which I am speaking. I am speaking of scientists who are only found typically in science-oriented organizations. They are doing the research in the universities or in the few industrial and governmental laboratories such as characterized this morning.

Now there is another group of scientists doing a very effective job in working more directly for technology-oriented organizations. There persons are the backbone of many very fine applied research and development laboratories throughout the country. Some of these persons are contributing to science also--really keeping up with the frontiers of science--and at the same time are often deeply immersed in the mission of the organization. This is also a very important group. It is not, however, the group we are discussing today.

In the case of the Air Force, there are two broad methods by which scientists are engaged under contract. One is through the unsolicited proposal route which we practice at AFOSR. Here we advertise broad areas of interest in which we sponsor work. These areas are selected to delineate scientific fields on the frontiers of science which hopefully will open up entirely new ways of doing things for the Air Force and also, but to a smaller degree, to delineate the supporting-type research which the Air Force needs to fill in technology gaps. The unsolicited proposal route is attractive to those scientists who are working in the frontiers of science, searching for fundamental knowledge, the use of which is usually not known in advance.

The other main route by which the Air Force engages scientists under contract is by sending out requests for proposals to do research in rather specific, albeit sometimes quite fundamental areas. This method of support attracts those persons who are typically oriented to problem-solving, although some of them are very deeply immersed in scientific pursuits also. This latter support method is not utilized by AFOSR.

I have tried to characterize the two communities of scientists with which the Air Force works. Both of them serve a very valuable function; however, they differ in the one important regard for the purpose of this discussion. The members of the first group which I described are more likely to be identified in a very intimate way with the frontiers of science. They are more likely to be persons who will first discover and

really understand new phenomena. The other group of scientists who spend more time on applied research and exploratory development activities can eventually pick up the understanding of these new phenomena, as they are passed on by those science-oriented people.

The AFOSR program is built on the premise that it is important to the Air Force to have involvement of a certain fraction of the community of science-oriented researchers--those who clearly have their professional interests in pushing back the frontiers of knowledge. The greatest asset of AFOSR is that it can and does bring proposals from the greatest scientific minds of the country, and that a very substantial number of these are brought under AFOSR support. I am speaking of those scientists who can have their choice of research support sources, including the NSF and private foundations.

We have heard earlier today how in outstanding industrial basic research laboratories it has been found possible to provide an environment which attracts and holds some of the country's most creative scientists, to support these scientists in frontier-type basic research with little obvious connection to the company's mission, but to simultaneously provide the company with very substantial benefits toward its mission. In a similar fashion we find that the Air Force support of the highly creative scientists who are attracted to AFOSR support, and are doing research, much of which has little direct relation to recognizable Air Force technology goals, bring indispensable benefits to the Air Force. Many of these benefits are over and above those accrued to the Air Force by the support of the same scientists by NSF or other research agencies other than the Air Force. The challenge to AFOSR management is the carrying out of its contract and grant administration in such a manner as to attract those scientists who clearly have their choice of support, to make it possible for them to carry out their chosen research in an uninhibited, expeditious manner, and at the same time maximize these additional benefits which accrue to the Air Force. At this point it is clear

that our management objectives and focus at AFOSR have a common thread with that described earlier by Dr. Goldman and Dr. Reiss.

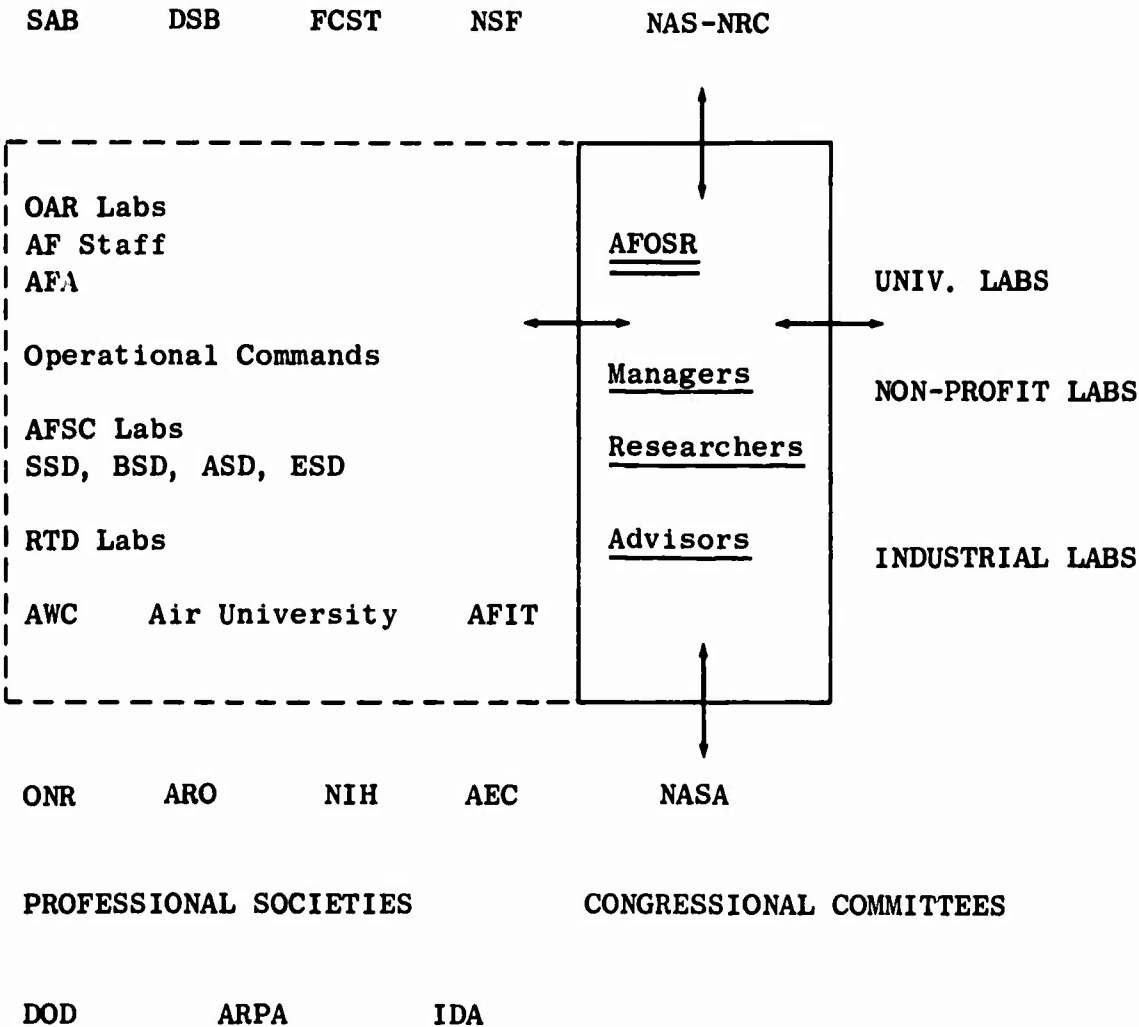
We have found it very helpful in thinking about AFOSR and its mission to describe it as a Research Institute as illustrated in Figure 1. We have our AFOSR staff (the "Research Institute Managers") and various advisors, and, of course, most important of all, the people who do the research. The AFOSR Research Institute can be visualized as a catalyst, interacting with both the scientific community and the Air Force. Both interfaces are, of course, very important.

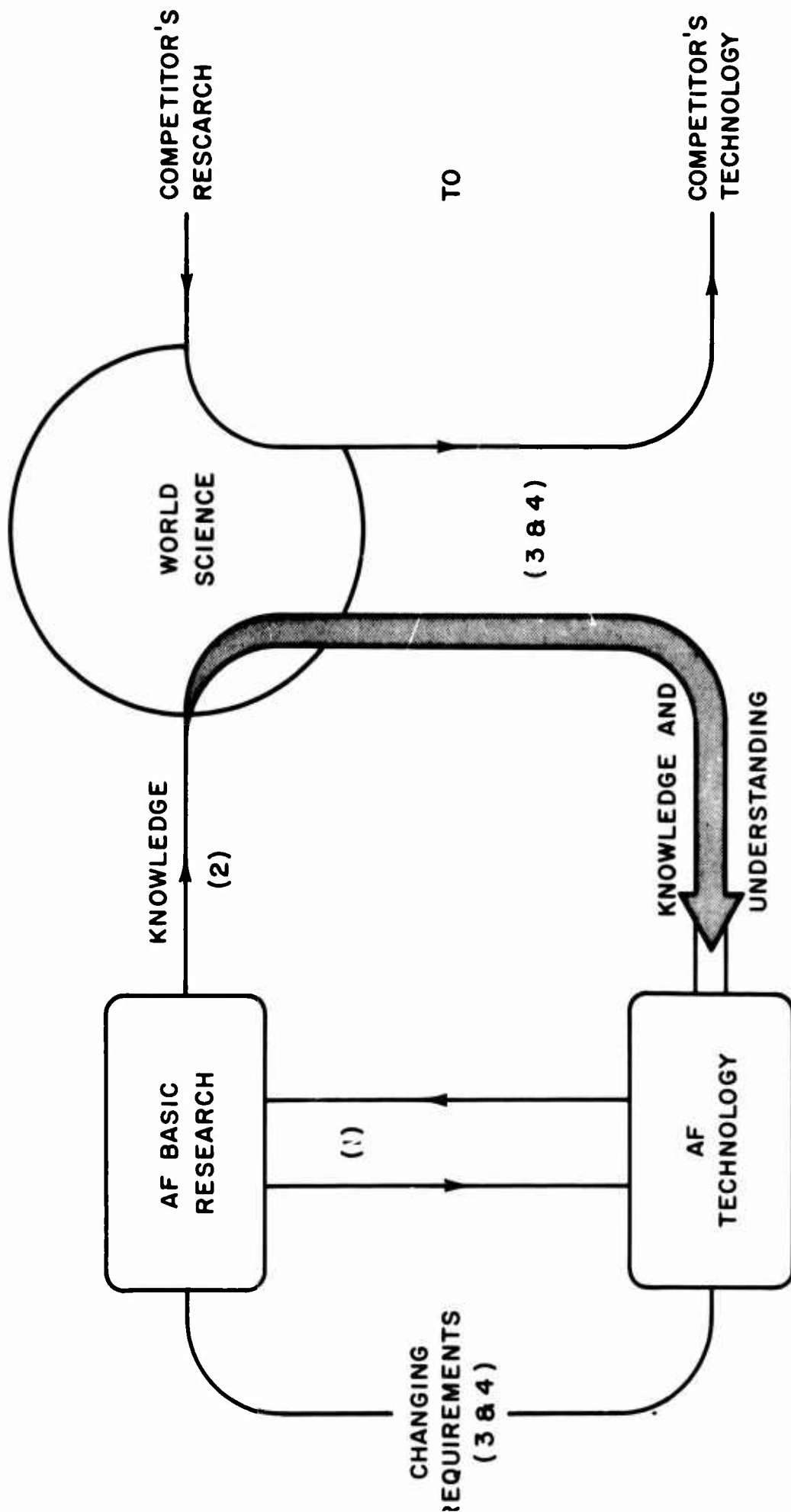
Figure 2 describes our mission in still a different way. Part of what we do is to look at technology needs and attempt to provide research results. However, the large bulk of what we do involves making contributions directly to world science, not knowing in advance what technology area, if any, the work will be pertinent to. In choosing areas to support we sometimes attempt to pioneer in new fields which offer significant promise for the Air Force, provided, of course, that the development of the field has reached the stage that there is reasonable hope that it can be colonized. Thus the choice of areas can affect the rate of development of what may be particularly pertinent areas. In other cases our support, along with that of other agencies, follows the development of that frontier of science, recognizing that as science continues to develop in its normal fashion it will always provide surprises for the Air Force.

Now this direct involvement with world science brings something very important to the Air Force, in addition to affecting the rate and nature of increasing scientific knowledge. This comes through the knowledge and understanding which can be brought directly to technology through consulting, participation on ad hoc groups with people with systems responsibility, etc., as illustrated in Figure 2. Not only do the results produced by AFOSR-supported researchers come out, but what is often much more important than that--these persons can act as a very effective information retrieval link, if you like, for a broad spectrum of science. This is

Figure 1

AFOSR AS A "RESEARCH INSTITUTE"





possible since they have a very intimate knowledge of the status of other work related to their own specialty--that is, they are members of the so-called "invisible colleges" of specialists. Now in any competitive situation, whether it be in industry or in military, the competitors are both drawing from the same body of world science, while simultaneously contributing to it. It may be in the long run that how well this part of the job is done determines who has the competitive edge.*

There is still another important aspect of this interaction with the agency's technology that is extremely important; this has to do with the feedback of needs to the research program.

Who better can understand the problem in scientific terms than the researcher himself if he really wrestles with the technology problem. Also, there is a very important motivational factor. If he gets intimate knowledge of the needs, he is much more likely to be motivated to do something significant about meeting these needs.

It's clear then that the interactions between the researchers and the Air Force technological community is an essential part of the AFOSR activity. Who is responsible for bringing this about, and how is it done?

The management responsibility for this coupling lies with the individual AFOSR staff scientists. This part of their function is essentially an open-ended one--that is, the opportunities are essentially limitless. It is one in which professional knowledge and ingenuity have a high premium.

* This function, which may be described as "a window between science and technology," has been discussed in detail in a previous paper, "The R&D Organization's Fundamental Research Activity as a Window between Science and Technology," AD 616834, Defense Documentation Center.

Regardless of the background with which an AFOSR staff scientist comes to the job, he must keep his contacts with counterparts in the Air Force applied research-exploratory development community current. Here a lot of personal contacts are made by visits, correspondence, special reports, program reviews, participation in joint task groups, etc.

Some of the most meaningful coupling activities are those which directly involve the research scientists AFOSR has under contract. While these contacts are strictly voluntary on the part of the contractor or grantee, we find that scientists around the country are ready and willing to participate directly in Air Force activities in many ways. A few examples include the following: trips to Air Force installations to perform consulting service; membership on ad hoc groups to study feasibility of various exploratory development programs; state-of-the-art reviews, either oral or written; special purpose symposia which are specifically designed to bring technologists and scientists together; special lecture tours; performance of feasibility studies on research phenomena to package them in a form more likely to be useful; and direct consultation with the aerospace industries.* Many basic research scientists find very significant satisfaction and stimulation as they make these important contributions directly to the stature of the Defense establishment, in addition to the important contribution which they are making by virtue of adding to the fund of basic knowledge.

Thus, it is seen that the Air Force utilizes its extramural research program, administered through AFOSR, primarily to support highly creative science-oriented persons doing research, the utilization of which is not always immediately apparent. However, the Air Force is directly benefited by the Air Force science-oriented activity both because the talents of

* For more details on AFOSR coupling activities, see "A Summary of AFOSR Coupling Activities."

very capable scientists are brought to bear on fields holding particular promise to the Air Force, and what is perhaps even more important, the Air Force support of scientists provides channels by which they can contribute more directly to the defense of the country by consulting, etc., than would otherwise be the case. In addition, this Air Force support provides a number of other benefits, albeit less direct, associated with the general strengthening of science, through having multiple sources of support available, and with the increase in the supply of graduate students and our ability to recruit them for Air Force activities, etc.

In summary, the Air Force is committed to the support of fundamental science because we believe that this support brings the Air Force very direct benefits that cannot be obtained through research which is closely allied to the end items nor by the support of fundamental science by other agencies. We are committed to the unapologetic support of research which is admittedly strongly science-oriented.

It pays very direct benefits to the Air Force. The major contribution of AFOSR is that we provide a mechanism by which highly creative science-oriented persons are involved in the Air Force program in manners which both they and we agree to be mutually beneficial. We are thus at least partially tapping this important potential for the continued strengthening of the defense of the country.

VII MANAGERIAL PRINCIPLES FOR PLANNING RESEARCH
FOR INDUSTRY AND GOVERNMENT

Lawrence W. Bass

Dr. Price (introduction for luncheon speaker):

It is a great pleasure to present Dr. Larry Bass as a participant in this program today. It has been a real privilege for the rest of us on the panel to work with Larry, both in the preparation of this program and during the proceedings today. He comes to us with a great wealth of experience, which is highly relevant to this symposium session, and he brings this background to bear on the subject at hand in a very gracious and effective manner.

Dr. Bass received his Ph.D. in chemistry at Yale and did postdoctoral research at the University of Lille & Pasteur Institute in Paris. His some forty-odd years of professional work range from very significant scientific contributions in biochemistry at the Rockefeller Institute to important executive management positions at Mellon Institute, The Borden Company, Air Reduction Company, Vice President of U.S. Industrial Chemical Company, and most recently as Vice President of A. D. Little, Inc. In the latter position since 1952, he has specialized in providing consultation on R&D management, serving many clients in industry and government. During this period he has been much involved in the overseas work of A. D. Little. Although recently retired, he continues as a consultant to A. D. Little, Inc., with an unbelievable agenda, which includes a special emphasis on the role of R&D management in the developing nations. We are now privileged to hear about this special interest of Dr. Bass.

Dr. Bass:

I'm very happy to be back today on an American University program on research administration. I was on one of the early courses when Professor Hattery started the series, and several times since then. It's always a pleasure to be here.

One of my major professional interests is in increasing the technical resources of developing countries and in encouraging their more effective use in socioeconomic development. These are vital in the planning and working out of programs for improving standards of living. I have often discussed the subject with Bill Price, and he asked me to make it the topic of my talk today.

I am happy to find that Jack Goldman and I share similar views on what developing countries should strive for in utilizing their trained manpower. He has referred to the heavy concentration of research in Pakistan on high energy physics. He feels this is not making optimum use of the talents of young scientists in directions which contribute to the welfare of the country. I agree. I think undue emphasis on highly theoretical subjects detracts from the vitally important problem of using science and technology for improving the national economy.

I hope no one will misinterpret what I am going to say about basic research. I am all for it, and a long time ago I spent several years doing it. I think that encouraging a high level of scientific thinking is a necessary part of training good research men. This mental attitude is imparted by the time-honored procedure of carrying out basic research on a thesis problem. But when it comes to putting these new talents and skills to constructive application for the national welfare, some sense of balance has to be injected. There is needed some realistic distribution of research effort between theory, representing training in the methodology of science, and practice, representing the development of new and improved technology.

The United States has a luxurious scientific and technical pattern. We can afford a heavy input into fundamental research. Nevertheless, we are concerned as to what should be the logical basis on which our scientific resources are deployed. The subject is of great moment to thoughtful people in government, industry, and the universities. How can we move in the direction of better balance in influencing the relative emphasis on different areas of science and technology? This approach does not imply regimentation of scientists according to a master plan. But it does involve finding means to encourage them to make voluntary selections of careers according to the needs of different areas. Actually this process already exists in number of job opportunities, salary levels, and conditions of employment.

Several educators have told me they are not happy about the scientific training and indoctrination we are giving to students from developing countries. We fit them into the pattern that has grown up as being suitable for post-graduate study in the United States. In their advanced work these foreign students are inspired to become dedicated to a very restricted scientific area. They are heavily oriented toward these same specific areas as desirable directions for their life work. Is this the best preparation to give them for making a contribution to their own countries when they return home?

Some educators even question whether our system of research training gives a sound philosophy for young scientists in our own country who enter applied research. Other speakers on this program have pointed out some of the problems of readjustment from purely scientific orientation to the world of practicality. There are no obvious universal answers. Certainly I am not going to propose one. But studies by social scientists, such as we are hearing about today, should lead to new ways of turning out thoroughgoing scientists who can more readily adapt themselves toward productive and satisfying careers in applied fields.

Turning back to the developing countries, the problem of utilizing scientific talents is more acute. In the United States we have flexibility in finding solutions. But in the newer economies overseas the acceleration of national plans for development is paramount. They are in the earlier phase of improving technology chiefly by importation from abroad. To meet their needs, they cannot yet wait on the longer process of building from within, relying on their internal technical strengths. But they must prepare for the not too distant day when they will have to develop more of their own technology.

The major new installations in developing countries are usually made by using foreign experts, and most of the equipment is imported. These experts are responsible for starting up operations, and then turning the plants over to local management, which has received on-the-job training during installation and start-up.

The emphasis is on immediate productivity along the original plan and design. The future need for improved technology is often overlooked because of concentration on getting the operation started. As time goes on, new sources of raw materials may require major changes in process or fabrication. The requirements for market satisfaction may become altered, either for domestic consumption or for export.

In passing, it is interesting to reflect that many plants installed in developing countries are not wholly in harmony with one of their chief objectives, namely, the creation of many new and better employment possibilities. Industrial jobs usually afford a higher wage and improved standard of living than the historic pattern of occupations. The new plants are often highly automated, reflecting the philosophy of design in mature economies. The number of new jobs created may therefore be disappointing. There is one advantage, however, the fact that modern equipment makes for better quality control. The need for sustained quality to conserve raw materials and to improve efficiency of operation is in the national interest in the newer economies.

The fact that technology must be dynamic is a built-in attitude in most American managements. Within the last few decades most enterprises have come to realize this requirement for survival and have made provision for the technical help they need to keep abreast of change.

These factors all point to a growing need for more scientists, technologists, and engineers in the industrial complex of a developing country. Supplying these skills is going to be recognized more and more by enterprise managers and government planners. But this cannot be brought about overnight. We know from our own long experience that a man with a background limited to theory is not ripe to supply the experience-based know-how to solve immediate practical problems most effectively. Therefore, these new installations need to begin now to plan for their future technical requirements.

Into this situation are being injected more and more men who have been trained abroad. They are influencing favorably the educational programs inside their countries. But they are often imbued with the idea of basic research along the directions of their thesis problems. The plants in which they might find employment are often located in spots that are at considerable distances from the cultural and prestige communities found in the capital cities. Further, the returned scientists frequently find that their foreign training has not opened immediately the doors to the type of employment they had hoped for. Sometimes they are greatly disappointed to learn that they may even have lost seniority in the government organizations which they rejoin. And if they are offered employment in industrial units, they may find that the managements do not know how to make best use of their skills and do not feel the need for long range research. This is not surprising, because even in our own country communications between management and technical personnel are often faulty.

I have finally reached the main point I am trying to make in this talk. Developing countries need to examine much more critically and

broadly their requirements for technical resources and the means of using them to best advantage. The experience over several decades in the United States and other highly developed economies can be helpful in this respect. We have done this in the United States without any formal organized planning for the nation as a whole, but instead have created our technical community by experience through trial and error, a process that continues actively. Some other countries have had overall national policies for government encouragement of industrial technology. Both methods can be successful, and both may have their merits in a particular situation. From this wealth of background in other nations, each developing country needs to set its own course to meet its local objectives and requirements.

The procedures for carrying out an assessment on a national basis for developing and deploying technical talents is the subject of a talk I am giving tomorrow at the Princeton University Conference on the Middle East. In it I am recommending a four-point approach:

1. A comprehensive survey of technical personnel, facilities, and organizations already available in the country.
2. A realistic appraisal through intensive study of the pattern of technical skills and experience currently needed by industry and government.
3. A matching of technical resources against needs and development of a program for reorientation.
4. A longer range forecast of technical needs and the formulation of policies and procedures to meet them.

I do not think that such studies can be carried out properly except from a broad point of view. Scientists who have only an academic and theoretical background are not good judges of the needs of industry. While economists and political scientists can set the broad directions of industrial growth that are desirable, they are rarely competent to judge

the technical requirements of specific industrial operations. Managers of enterprises are rarely competent to prescribe the detailed programming of technical activities. Industrial technologists are often too thoroughly immersed in their own specialties to have the necessary broad view of national objectives and managerial requirements. It is my thesis that it is only through combining these four essential categories of thinking that the wisest plans can be developed.

Techniques have been worked out in mature countries for obtaining useful information on technical resources, and these are available for adaptation to the needs of developing countries. There are also highly developed skills, based on experience, for determining the technical requirements of industrial operations, either on the scale of an individual enterprise or a broad complex of industries; these are not hard-and-fast rules, but guidelines for establishing ranges. There are also techniques for estimating the distribution of technical talents in terms of projected requirements. There are improved managerial techniques for the development of general technical policies and the programs required to implement them. If this reservoir of skill and experience had been available in this country forty or fifty years ago, it might have had a very real effect on the efficiency with which we have used our technical resources.

There are pitfalls in trying to develop technical resources in the newer countries unless the concepts are realistic. As an example, many developing countries, in fact most of them, have economies that are based largely on renewable resources, such as agricultural products. Hence, the fields of food science and technology, and biochemistry are obviously of much importance, and many promising young men have been sent abroad to carry out advanced study in these areas. I question whether this training has been put to optimum use in these countries. There is a tendency to centralize these scientists in research laboratories where they carry out programs of research which superficially are on subjects of practical

interest in their local areas. At the same time there are beginning to spring up many food manufacturing establishments which are inadequately backed up by technical assistance. While food technology is a very important branch of science, I think that it can be overdone by failing to match the use to which the knowledge is put with the requirements of the particular economy.

I offer this example of food technology with some trepidation, and I am sure that some people will disagree with me. I am doing it to point out that it is possible to get an imbalance in technical emphasis even in areas which seem to have very practical significance. The same thing could be true in respect to overdoing the emphasis on polymer chemistry, metallurgy, or textile chemistry, at the expense of other areas important to the country. In other words, there is need not only to avoid excessive concentration of training programs on highly theoretical subjects, but also to make realistic appraisal of the pattern of scientific and engineering disciplines that need to be reinforced for the balanced development of the economy.

As a concluding topic I would like to turn to the question of improving managerial skills for technical activities. The question is often asked, how do we in the United States train research and development directors? Actually, we do not do a great deal of formal training. It is largely up to the administrators who are arising in technical organizations to train themselves. But more and more aids are becoming available for this self-education in good managerial practices. There is a voluminous literature on research management, and the volume is increasing. There are training courses such as the present one. There are many symposia being held by various technical organizations. This is all in the right direction, and through the efforts that are being made by many companies to give members of top and middle management better facilities for developing their skills, I am sure that the next ten years will see a great improvement.

There is a very great need for improving managerial skills of all types in the developing countries. There is already a strong trend toward institutions to foster courses in business administration, often along the lines of those that have become so well established in the United States.

From many sources we hear that there is great need for turning out better managers of technical activities in these newer countries. I am convinced that this is true, and I am continuing to do what I can to call attention to this vitally important topic. One of the most rewarding experiences of my life was organizing and participating in a course given in Cairo about a year ago to 38 Egyptian technical directors. This was a resident course consisting of forty two-hour sessions, half organized presentations and half small group working discussions. It was carried out under the sponsorship of the Ford Foundation and the U.A.R. National Institute of Management Development. I am happy to say that it would be hard to find a more intelligent, hard-working, and dedicated group of men than I met on that occasion.

Here is an area that presents an exciting goal to American managers of science and technology. How can we best interpret the results of our experience, so that our counterparts in developing countries can use this background to increase their contributions to their national economies?

VIII QUESTIONS AND ANSWERS

Dr. Vollmer:

We are now open for questions. Let me stop first and ask the members of the panel if they would care to ask questions of each other. Are there any questions that you would care to ask of anybody else before we open it up to those in the audience?

Dr. Goldman:

I would like just to make a comment. I've been managing research for a considerable number of years and like to take some credit for having helped put Ford on the map in research, but I don't think I have ever heard a more cogent and critical analytical presentation of the relative ways to handle what Howard Reiss called "phenomena-oriented" versus "applied research." I surely hope that this is somehow going to get published, so that people like me and my management and colleagues would be able to point to this kind of thing. My colleagues in other companies have sometimes found that they can pontificate a great deal on how to do basic research, but they always run into difficulties in coupling to the applied people because they miss some of the tenets that Howard pointed out which are very essential to the administration of applied research.

These are two different breeds of animal, and one has to gain a perspective on these two parallel paths in research that you travel in different ways.

Question (from the audience)

Dr. Marquis mentioned, and it is pretty well known in business, that there is a big gap between the master's degree and the Ph.D. degree.

Certain universities are taking some action to fill this gap by offering a doctoral degree in engineering. I wonder what effect such a program, if widespread, would have on science and technology?

Marquis:

Well, there's not much change in science because the Ph.D. has been a requirement for scientists for a long time. But there is a rapid, although small, change in engineering in that more universities are giving more doctoral degrees in engineering.

Dr. Reiss:

It's true, the engineers are beginning to get more doctor's degrees. Some engineers, particularly the so-called old line engineers, are disturbed by the fact that so many engineering departments are turning into engineering physics and engineering science departments.

You go to an electrical engineering department and you find that they are not working on transmissions or generators; they're working on material science, semiconductors, on insulators, and on metals. If you go to an aeronautical or mechanical engineering department, they are working on energy conversion, plasmas, gas dynamics, and so forth. I think this is good, provided that the engineers themselves make sure that they employ in these departments people who are engineers, rather than scientists, for the following reasons: that there are very few applied scientists who are trained at the universities. The reason is that most professors of science will argue that they can't train Ph.D.'s by having them work on an application problem for a thesis. I think that is emotional, not real.

So you get what I call "the secularly immature scientist" coming out. On the other hand, the engineers themselves have always regarded as a perfectly acceptable, creative act to synthesize something new out of existing knowledge. If they want to turn their attention to applied

science, buildings, transistors, and things like that--fine, because that's where we will get our applied scientists from.

So the Ph.D. in engineering, which converts a man to an applied science, is good. If they hire physicists or chemists to staff these departments, then they will defeat their purpose. It will just make more of a strain.

Now, with respect to the question concerning science, in our laboratories--the phenomena-oriented labs--we try not to hire the overtrained technician or the undertrained scientist. They get to be very unhappy after a while.

If a person in our lab who is a technician goes out and get himself a Ph.D. degree, he loses a job, essentially, because he has to apply all over again. He doesn't automatically graduate and become a number of the staff. Now there are people without Ph.D.'s who are equivalent to them--the so-called "self-made scientist." You can recognize them pretty well in terms of accomplishments. But the intermediate trained man, I don't think is very good for a phenomena-oriented lab. He has to have the equivalent of Ph.D. training.

Question:

May I ask, Dr. Mann--you may have covered it but I may have missed it--how did you determine which of these units were most effective and which were least effective?

Dr. Mann:

We asked a half dozen top people that everyone had said would know, because of the very careful review process which was used in this agency, to rank all of the randomly selected units out of each of the divisions on the five dimensions of performance that we used in our study.

Question:

Was there any other quantitative measurement of performance used?

Mann:

No.

Question:

Then you based your study on performance measures that are just conclusions of management.

Mann:

Yes. Let me say it this way. It's just the same intuition that those same men use in spending hundreds of thousands of dollars every year, in deciding who gets what research laboratory equipment, who gets what monies to start new projects, whom they go out and hire in the field, which programs get to go ahead, which ones slow down, etc.

Dr. Bass:

You also asked the research groups to evaluate themselves, didn't you? This was an additional source of information on performance to correlate with management judgments, wasn't it?

Mann:

Yes. The section heads and the nonsupervisory scientists agreed. The correlation is in the order of .55.

Question:

My question is to Dr. Goldman. I believe it also covers the other people as well. The discussion about "science" implied "physical science." Is that your understanding? Was that the term of reference or is Ford

concerned at all with the styling of automobiles, and personal choice factors in behavioral science, and so on?

Goldman:

To answer your question, it was not my intention to concern myself exclusively with physical science in talking about a scientifically oriented research institution. Everything I said applies to, say, the pharmaceutical industry in which they would have research laboratories engaged in basic research in the life sciences. So you may want to distinguish between physical science and life science, but I think you will find what we have said applying across the board in the areas of pure science. I would be quite reluctant, however--with all due apologies to our vice president for styling whose name is William Ford and who also owns the Detroit Lions--to call any aspect of the styling of a car "a science." It's an art, and I think they would insist that it's an art. Over the years we have been trying to instill a little feeling for engineering in the hearts and minds of the stylists, but it is still an art.

Now there are other aspects of research in different parts of a corporation such as ours. The market research people, for example, rely heavily upon statistical science, and we have two experts in statistics, and that is certainly a science. As a matter of fact, the director of market research at Ford is a man who was a professor of statistics at the University of Chicago before he came to Ford. He has a small group working with him which does what I would be happy to call "phenomena-oriented" or "fundamental research" on new mathematical techniques related to computers, endeavoring to use computers in revamping the approaches and the thinking toward market research analysis. Of course, the University of Michigan is one of the pioneers in this field as well. I think I have answered your question.

Vollmer:

Dr. Marquis had a comment on that question.

Marquis:

Probably to the extent that behavioral science is a science, it will follow the same picture we have been describing for the biological and the physical sciences. As a small piece of evidence, Chambers recently published a study in Science (Sept. 11, 1964) in which he compared a hundred eminent chemists--"eminent" meaning members of the National Academy or starred in American Men of Science--with a hundred other doctoral chemists who were not distinguished but were matched on age, education, and so forth. He made the same comparison for psychologists--picked a hundred recognized as eminent and a matched group of a hundred who weren't, and found that the same characteristics distinguished the eminent from the less noted scientists in both groups.

Bass:

I know a number of the larger industrial research laboratories which are injecting social sciences into their organizations, and so are we, Stanford Research Institute, and others. On our staff we have several psychologists, anthropologists, and other social scientists to broaden the base of our approach to problem solving.

Vollmer:

That is the case with SRI also.

Question:

In relation to what one of you said earlier--I see around on every hand in the area of technology the profound influence of the kind of people I would broadly call economists--both on the macroscale, which I

certainly have noticed in the Pentagon, and on the microscale, when I see the growing interests in the utility of what I would say is "dynamic modeling." But the only word I heard here today about economists was that they don't have an appreciation for technology. But what is the role of the economist now in this research that you have been talking about today? I don't want to hear about it historically--we've been over that ground. Let's get down into research. Would anyone address that?

Bass:

I have very profound admiration, respect, and belief in the economic sciences. There has been a revolution in the economic approach from the classical descriptive form into quantitative econometric analysis. This strong trend is relatively recent; it blossomed in the 1930's. It has revolutionized the concepts and made economics a science rather than a branch of the liberal arts. Certainly industrial companies are using economists more and more, and they are interrelating them with the people in the technical departments.

We have a sizable number of economists in our organization--SRI has, and so on--who fit in extremely well. Now I was the one who said economists alone can't be expected to have enough grasp of strictly technical problems, and I therefore made the point that it takes four interlocking types of specialists to come up with a comprehensive plan for development, for instance on a national scale--the technologist, the industrial manager, the economist, and the planner or political scientist. National planners may step out of the role of political theorists and overlook the technologic backbone of the national economy. Then they become divorced from reality and follow unilateral theory, and they need to have the reins pulled in by the technologists, the industrial managers, and the economists.

Vollmer:

Dr. Goldman, did you have a comment there?

Goldman:

Speaking from my experiences in our company, while I cannot say that the economists have played a direct role in the act of research or its management, I will say unequivocally that the best friend that science has had in our top management echelons have been the economists, bar none.

I think our economists have been much greater friends of the philosophy of scientific research that we have been espousing and have been much stauncher supporters than our engineering vice president, who is an older engineer who really doesn't appreciate science to the same extent that our controller, Mr. MacNamara did when he was president, our chairman of the finance committee, and our finance vice president.

Reiss:

I think the answer is reasonably simple. It again depends upon the business of the particular technical organization under consideration. If its business deals with a field of economics, then you could very well have a phenomena-oriented research economist or econometrician, I suppose. For example, in our company we are a systems company, and we are looking beyond the time when we'll deal only with, let's say, the space program or the Department of Defense to the time when we begin to pay attention, perhaps, to city planning, or to assisting underdeveloped countries. These are also systems activities.

We have a program with the state of Oklahoma to do a systems analysis of its school system. The systems approach to public education, for example, involves a lot of economics, and so forth. So if a company gets embroiled in that particular area of the business, there is no reason why they shouldn't have phenomena-oriented economists to do research in their

laboratory. But if they aren't going to do anything like that at all and they never intend to, then there is no reason to, or there's not much reason to.

Question:

I think in the past couple of years there have appeared several articles--I think one in Fortune magazine, which unfortunately I can't identify further--which deal with the disenchantment of certain industrial managements with their efforts in the field of research. Apparently in the 50's, everybody thought it was a wonderful thing to get into and build research organizations. And in the 60's they are beginning to do a double take with this. Would anybody care to comment on what's happening in this regard?

Reiss:

Actually, I think the problem lies in the fact that not every business needs a research organization. Even if they need the research organization, they better not have it unless they figure out what they are going to do with it. It depends upon whether the company is large or small, whether it's diversified technologically or not, whether it's consumer-oriented or systems-oriented. I can think of some very good businesses, some of the best in the country, which really don't have research of the form we are talking about. They probably should not have it.

During the 50's there was a fad to have a research laboratory, even if you didn't know exactly what you were going to do with it. And there was the stimulus provided by the example of the transistors, which seemed to exemplify, in the highest form, phenomena-oriented research being married to application and to business. In this era, we see the optical maser, for example. And a lot of money--hundreds of millions of dollars--has been spent on the optical maser. This was supposed to be this

decade's transistor, so to speak. That hasn't happened. The optical maser is a fine scientific accomplishment; it's a device that won the Nobel prize; and it actually is useful in specialized scientific application; but it has not yet achieved systems status like the transistor.

Vollmer:

This relates to an earlier question that I raised, I believe, and that is, when should a company, or under what conditions should a company, have this kind of a fundamental research activity? Would you say that it's any company that has some plan as to how they might use this research, or whether they are large or small, or is there a certain critical size, or can you provide any kinds of guidelines on this?

Goldman:

I'd like to tackle this one, and in the process I'd like to evolve the theory whose origin I owe to Dr. Larry Hafstad, who is the research vice president of another motor company that makes another line of car that happens to build twice as many as we do--he had a very interesting analysis which I have embellished, if I may go to the blackboard for a moment.

I'm glad that you ask this question because I think this raises a most interesting point. You can plot on the ordinate the percentage of sales spent on research against some figure which will measure the longevity of the product--how rapidly a product becomes obsolescent or how rapidly you must introduce new products to maintain your position in the market place. You get a curve something like this. At the top of the heap is an industry such as the drug industry. They have to plow back large amounts of money into research because in the current phase of drug research there are new drugs coming out regularly, and you just don't hold your head above water if the competition runs away with you. So, some of you might have something like 15% in research in the drug

industry. A little bit below that is the electronics industry, which would be something like 11%, and somewhere down here might be the electrical industry, which is about 9%, and then comes the chemical industry which might be 6 or 7% invested back into research.

Then you get into the hard materials--steel, and say, automobiles, which may run about 3%. And now of course, it's always hard to identify what's research and what's not research.

If you draw this very straight line--Hafstad called this to my attention--it fits like a glove, and if you have had enough statistics, enough data, you can analyze this and look at Parke-Davis and they may do 300 million dollars worth of business but they plow back about 45 million dollars into research and development in that industry. To this I add that if you, as a general rule of thumb, assume that any industry that fits somewhere on this curve plows back that percentage of its sales that it puts into research, and then plows back that percentage of research funds into basic research, you have another good rule of thumb for which companies do, or which companies should do fundamental research. It does not necessarily mean that you have to be either large or small. It depends on the kind of product and it's what you are going to do if you do such research. The case that I'm familiar with is Parke-Davis, where they do perhaps a 300 million dollar business; they put 45 million dollars into research, and another 5 million or so into basic research.

Now, you can be a 100 million dollar company sitting up here in the electronics area, or the pharmaceutical area, and you have to plug a few million dollars into research.

Here is a little 300 million dollar pharmaceutical company putting as much into basic research in this sphere as, say, our laboratory in the automobile industry. In the Ford Motor Company, we are in a type of longevity business, a lack of technological obsolescence kind of business, in which with 10 million dollars sales, we will only plug back 3% of 3%

into the basic research aspect. You will find that if you take cases that you are familiar with, it fits pretty well. And then take companies that you know that should be on this curve, who don't plow it back, and by and large there is usually a sad tale of woe of falling behind the marketplace. In a few instances, companies that do plow enough of it back into research strike it rich.

Question:

Conversely, above that curve on the board, will they capture more of the market, do you think?

Goldman:

I think they have a good chance of doing so. Xerox is a case in point, there.

Reiss:

I think I would just like to make a parenthetical remark about what I said before. Although the establishment of research laboratories was a fad in many cases during the 50's, it's now a fad to disestablish them. To be carried along with that is dangerous, because just as some companies don't need research laboratories, some do. And because everybody is dropping them, they'll think they have to drop it when they will really need it. It's very important to inspect each case in detail to see whether or not there is function for research or not.

Marquis:

I think there is a good reason why there are fads in research expenditure. Fads flourish where facts are scarce. The value of research is almost impossible to measure, and if you can measure it, it has a long time lag. The feedback of results in some industries is 15 or 20 years.

In other industries, like pharmaceuticals, it may be only three to five years. This may be one of the reasons why railways and steel, for example, in which there is a long delay in the feedback loop for results, do not find it as easy to decide to invest heavily in research as drug and chemical firms do.

Bass:

In regard to that article in Fortune--I don't think the slant was expressed quite as that article has been interpreted. First of all, I don't know any company that is really making a serious curtailment on research. On the contrary, they are continuing to expand. There are companies which went into basic research without the benefit of such wise men as we have here in Jack Goldman and Howard Reiss and their management. These other companies went in on the false theory that if you wanted to make scientific breakthroughs, all it took was to collect a bunch of scientists and let them work on anything they pleased--"serendipity"--you just left it up to the scientists and they would come up with something. There are so many obvious fallacies to this that I won't pursue it any further.

Now what has happened is that a great many companies which started in improper directions regarding management conceptions of what they could get out of basic research have begun to revise them. In some cases, they have done away with a corporate basic research laboratory and have redistributed the effort into applied research. But I don't know anybody who is cutting back on the total research effort. I think they are reorganizing it more in terms of what is appropriate to their company needs.

Goldman:

I think of another key factor that Howard Reiss told us the last time he visited us: "How well a corporation can deploy its research efforts, take advantage of it, recognize it, and implement it really depends, in the

final analysis, on something we really didn't touch upon, and that is the attitude of its top management--the understanding of its top management as to why it wants research; how it measures the value of research. Does it understand all these things we enumerated of the whys of basic research, or does it just want to have a transistor presented to it on a silver platter?"

Now, in Howard's business--systems activities in the modern day--you may want to embellish this yourself. As he pointed out, his top management feels they have to have a grasp of nonclassical kinds of things. They have to understand what relativity means, because you start dealing with relativity when you're talking about space shots, when you're talking about interplanetary travel, and so on. So you have to have some liaison with the nonclassical world. If the top management does not have liaison with the nonclassical world, then it will never grasp the next generation and the further generations of the vehicles or systems, or what have you. Therefore, his top management calls upon him to instruct them in what's going on at the frontiers of science.

The last time I made a presentation of our program to my top management, I enumerated some of the important advances in science and then showed some of the technologies that had come out of our science. When I finished, Mr. Ford, Henry Ford, stops me and he says "Look, I really don't grasp the innuendos of this thing you call 'superconductivity' or 'electrochemistry', but I do know one thing, that if I'm going to measure how good you guys are, I want to know how much you publish in journals that are refereed." Now, that's the chairman of the board of my company, not one of the scientists or scientific managers. When the chairman of the board knows that the way to measure this is to measure your productivity in refereed journals, boy that's half of your battle. He understands what he expects to get out of it.

Question:

Larry, do you know of any company in a business of over a billion dollars that has either eliminated, or plans to eliminate, its research function?

Bass:

No, very definitely I can say that I do not. There are rearrangements in technical functions and staff, and the "research and development department" may disappear from the organization chart, but the technical manpower is for the most part distributed elsewhere in the company.

This goes back to the administrative policy of a company. Does it wish to have a central research function to carry out longer range work, or does it wish to decentralize the technical program and place it under the control of operating divisions? Some companies alternate between the extremes, in the course of a few years.

Reiss:

Actually, the question of elimination of research doesn't have to mean the disappearance of an existing laboratory entity. The mode of operation and management philosophy can change so much, that, although they call it "the research laboratory," it's actually an entirely different thing. And that sort of thing might happen. There are some organizations which are cutting back in that sense. Some very large ones.

Vollmer:

Thank you, gentlemen, for your questions and thank you for inviting us.

Appendix

DATA ON THE ORGANIZATIONAL SEPARATION OF RESEARCH FROM DEVELOPMENT

Howard M. Vollmer

Three of the speakers in this symposium are directors of fundamental research organizations that are organizationally separated from, and independent of, development functions in their parent corporations or agencies--Howard Reiss of the North American Aviation Science Center, Jacob Goldman of the Ford Motor Company Science Center, and William Price of the Air Force Office of Scientific Research. In this regard, Dr. Goldman has written that organizations devoted to fundamental research (or what Dr. Reiss calls "phenomena-oriented" research) can be quite useful to corporations that depend upon rapidly changing technology, but that:

Good research has to be cushioned. Perhaps I phrase it best if I say that good research must be insulated, but not isolated. It has to be insulated, or cushioned, because once people learn that they can utilize this talent to put out fires, to help solve immediate problems, then the research is crippled. It is for this reason that we are set up as we are--with basic research separated organizationally, but not geographically, from applied and product research.*

Jack Morton has pointed out that the Bell Laboratories have a similar organizational barrier between fundamental research, on one hand, and applied research and engineering, on the other:

* J. E. Goldman, "Basic Research in Industry," International Science and Technology (December 1964), p. 44.

...we want some feedback, so let us see how we get it from, say, applied to basic (research): We get it in one way with a space bond--people in applied and basic live in the same building. And we get it through a common language. But at the same time, we see that if applied people or engineering people can dictate what the research people do, they will kill the long range basic research. So we need an organizational barrier: One man--Bill Baker--is head of all basic research; other men head up applied research and engineering. Our people are free to sell, to stimulate and motivate all they like. But my engineers, for example, cannot tell the basic researchers what to do. And conversely, the basic researcher who believes he has made an important discovery cannot order the applied research or engineering people to pursue it. So this organizational barrier provides freedom for basic research and freedom regarding what shall be developed.*

We now have some data on the organizational characteristics and on the productivity and attitudes of research scientists in organizations that separate fundamental research activities from development in comparison to those that do not. These data have been taken from a nationwide survey of research scientists in four disciplines--chemistry, physics, biology, and mathematics--which has just been completed by Stanford Research Institute, under partial support from the Air Force Office of Scientific Research.† These data are presented here in order to supplement findings reported by the other social scientists in this symposium--Dr. Floyd Mann and Dr. Donald Marquis.

Table 1 shows data indicating that the majority of scientists in industry and the Federal government are generally employed in contexts

* J. A. Morton, "From Research to Technology," International Science and Technology (May 1964), pp. 88-90.

† A descriptive report of the main findings from this survey, along with a discussion of its methodology, is available in H. M. Vollmer, Work Activities and Attitudes of Scientists and Research Managers: Data from a National Survey (Menlo Park, California: A Stanford Research Institute report to the Air Force Office of Scientific Research, 1965)

Table 1

THE ORGANIZATIONAL RELATION OF RESEARCH TO DEVELOPMENT IN DIFFERENT
CONTEXTS: DATA FROM A NATIONWIDE SURVEY OF RESEARCH SCIENTISTS

<u>Type of Employing Organization</u>	<u>Research is Organizationally Separate from Development</u>	<u>Research and Development Combined in Same Org.</u>	<u>Development Not a Major Function in Employing Org.</u>
University (N=1942)	10%	8%	76%
Federal government (N=382)	23	38	38
Nonprofit corp. (N=273)	23	24	52
Industrial firm (N=1030)	40	54	5
Aerospace (N=127)	36%	59%	6%
Atomic energy (N=58)	48	48	3
Petrochemical (N=250)	30	68	2
Electronics (N=145)	60	39	1
Food and drugs (N=149)	40	56	3
Other manufacturing (N=222)	43	56	1
Nonmanufacturing (N=69)	30	26	39

Note: Totals for rows do not always equal 100% because of nonresponses or rounding off.

in which research and development are combined in the same organizational unit. In the electronics industry and in non-manufacturing firms, however, scientists are more likely to be employed in contexts in which research is organizationally separated from development. Equal proportions of scientists in firms in atomic energy activities are employed in each type of context, as is true essentially of scientists in nonprofit corporations (the majority of whom are in the "national laboratories" operated under AEC contracts). As would be expected, most university scientists work in contexts separated from development activities because development is not a major function in their employing organizations.

Table 2 provides further data which are indicative of the characteristics of organizations in which research is separated from development in contrast to those in which the two kinds of activities are combined. These data suggest that those organizations in which research is separated from development are more likely to employ scientists in fundamental or basic research activities (oriented primarily toward contributions to scientific knowledge), to allow these scientists a large degree of freedom in selecting their own research assignments, to employ them in single-discipline or one-man research activities rather than in multidisciplinary teams, to employ scientists with doctor's degrees, and to pay them higher salaries.

Table 3 shows data on what employing organizations apparently obtain as a result of the above combination of organizational environment and personnel characteristics. In those contexts where research is organizationally separated from development, as might be expected, scientists are more likely to make more contributions to knowledge in their scientific fields, judging from the numbers of professional journal publications produced, and to make more notable contributions, judging from

Table 2

CHARACTERISTICS OF ORGANIZATIONS THAT SEPARATE RESEARCH FROM DEVELOPMENT
COMPARED TO THOSE THAT COMBINE RESEARCH AND DEVELOPMENT IN THE SAME UNIT

	In contexts where research is organizationally separate from development (N=744)	In contexts where research and development are combined in same organization (N=972)
Scientist is mostly engaged in:		
Research oriented primarily toward contributing to scientific knowledge	48%	27%
Research oriented primarily toward solving problems of industry or government	23	46
Research oriented about equally to both	27	27
Scientist "definitely" experiences:		
A large degree of freedom in selecting research assignments	50	33
Scientist presently working mostly:		
In collaboration with others from different disciplines	26	34
In collaboration with others from same discipline	36	33
By himself	37	33
Scientist has a doctor's degree	77	67
Scientist has a salary of over \$15,000 a year	51	38

Note: This table and Table 3 show data from the nationwide survey for those scientists in employing organizations in which development was reported to be a major function, excluding those who reported that development was not a major function.

Table 3

BEHAVIOR AND ATTITUDES OF SCIENTISTS IN ORGANIZATIONS THAT SEPARATE
RESEARCH FROM DEVELOPMENT COMPARED TO THOSE THAT COMBINE RESEARCH AND
DEVELOPMENT IN THE SAME UNIT

	In contexts where research is organizationally separate from development (N=744)	In contexts where research and development are combined in same organization (N=972)
Scientist has produced 5 or more publications in professional journals during past 5 years	51%	39%
Scientist reports his work has been cited "fairly frequently" or "very frequently" by colleagues in other organizations	41	31
Scientist reports feeling a "strong obligation" toward the long range goals of his employing organization	61	61
Scientist has been with his employing organization for 5 or more years	56	54
Scientist hopes to remain with present employer for at least 10 more years	66	62
Scientist feels that management has given his work "the recognition it deserves"	81	74
Scientist is "satisfied with his job in general"	64	58
Scientist has job-related contacts with people responsible for product development, manufacturing, marketing, etc., "several times monthly" or more often	18	37
Scientist reports he "definitely" has had "opportunity to help translate research findings into useful applications"	26	34

the frequency with which they indicate that their work has been subsequently cited by scientific colleagues in other organizations.*

At the same time, Table 3 also shows that there is little, if any, difference in the two kinds of contexts with regard to the degree to which scientists develop a sense of obligation toward, or commitment to, the goals of their employing organization, although there is a slight tendency for those in the context in which research is separated from development to be more likely to feel that management has given their work "the recognition it deserves" and to say that they are "satisfied with their jobs in general."

Finally, Table 3 shows--again as would be expected--that scientists employed in contexts where research is organizationally separated from development are less likely to have job-related contacts with non-research people in product development, manufacturing, marketing, and other organizational functions, and are consequently less likely to be able to participate directly in helping to translate their research findings into useful applications within their employing organizations. There is a greater need in these separated units, therefore, for the assistance of research managers, liaison engineers, and other personnel who assume a particular responsibility for translating research findings into useful applications as a major part of their job. Other data from this nationwide survey show that research managers do indeed play an important role in "coupling" research and non-research activities in such situations. While Table 3 showed that only 18 percent of the scientists

* Other data, not shown in Table 3, indicate that this relation between organizational context and scientific productivity still holds, although to a somewhat diminished degree, when the productivity of scientists in the two types of contexts is compared while controlling the different proportions of scientists with doctor's degrees in each context.

(nonsupervisory) in contexts in which research is organizationally separate from development reported that they have job-related contacts at least "several times monthly" with people in their corporations who are responsible for product development, manufacturing, etc., further analysis also reveals that 68 percent of the 158 research managers surveyed in these same contexts have job contacts with non-research personnel at least "several times monthly." In other words, research managers are almost four times as likely to have such contacts as are nonsupervisory scientists. Thirty-nine percent of these managers reported that they had such contacts at least "several times weekly," and 20 percent said "daily."

In sum, these data support the claims of the speakers in this symposium regarding the advantages of the organizational separation of research from development in those cases in which a sponsoring corporation has the requirement for, and the resources to support, high quality fundamental research activities. At the same time, these data also support the need that all the speakers have recognized to establish "bonds" linking research with development, as well as "barriers" between the two. In many cases, research managers play an important role in establishing this linkage, while still protecting the integrity of the research function.

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